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Psychological Review

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HERBERT S. LANGFELD
PRINCETON UNIVERSITY

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THE PSYCHOLOGICAL REVIEW

AN OPERATIONAL RESTATEMENT OF G. E. MÜLLER'S PSYCHOPHYSICAL AXIOMS

BY EDWIN G. BORING

Harvard University

Psychophysical parallelism was basic to so much of the conceptual thinking of the experimental psychologists of the later nineteenth century that the principle then seemed almost axiomatically valid, like the laws of the conservation of energy and the conservation of mass. "No psychosis without neurosis" was good scientific sense in psychology, and the interactionist was necessarily suspect because the general acceptance of the physical world as a closed system made it appear impossible for an immaterial (mental) event to become the cause of a material (physical) one. At any rate there was no great opposition when Fechner, Helmholtz, Mach, Wundt and Hering assumed that every mental process must be correlated with a process in the brain, and that the nature of one must imply something about the nature of the other—at the very least that change in one must mean change in the other.

Thus one can say that the new physiological psychology was actually based upon the principle of psychophysical parallelism or, in more modern phrase, psychoneural isomorphism. This note is not concerned, however, with the history of this concept, which has been presented elsewhere,[1] but with its apparent axiomatic nature and with the question of what happens to a dualistic axiom when it has to be

[1] A. Bain, *Mind and Body*, 1872 *et seq.*, chap. 6; also his The correlation of mental and nervous forces, in Balfour Stewart, *Conservation of Energy*, 1874, 205–236 (parallelism was favored against interactionism because the latter was held to violate the law of conservation of energy); R. Eisler, Parallelimus, *Wörterbuch der philosophischen Begriffe*, 2nd ed., 1929, II, 377–386.

restated by a monistic positivist. In 1896, when G. E. Müller brought his thorough analytical mind to bear upon the problem of the physiology of color vision, he felt the need for a definite formulation of these fundamental psychophysical (he meant, *psychophysiological*) axioms.[2] He formulated five, of which the first three, the more general ones, are reproduced below.[3] These axioms lay down the general principle that a change in sensation is correlated with a change in neural process, that a difference in the kind or direction of sensory change implies a difference in the kind or direction of neural change, and conversely.[4] Köhler's postu-

[2] G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Z. Psychol.*, 1896, 10, 1–82, esp. 1f. Müller here (p. 5) also gives the relevant references to some of the other pioneers in the 'new' psychology who had expressed themselves on this matter: Lotze, Fechner, Mach and Hering. Mach laid down a 'heuristic principle' like Müller's axioms: "To every psychical there corresponds a physical and conversely. To like psychical processes there correspond like physical, and to unlike unlike." E. Mach, Ueber die Wirkung der räumlichen Vertheilung des Lichtreizes auf der Netzhaut, *Sitzungsber. Akad. Wiss. Wien*, math.-naturwiss. Cl., 1865, 52 (2), 303–322, esp. 320.

[3] The fourth axiom laid down the distinction between intensive attributive series (with zero at one end of the scale) and qualitative attributive series (with no zero at either end of the scale). The fifth axiom was concerned with the production of intermediates by the mixture of two extreme processes.

[4] *Cortical gray* is a familiar example of Müller's use of the axioms. Having established the system of colors without an absolute zero but with every color being some degree of black or white, blue or yellow, and green or red added to a constant gray, he inferred the existence of some constant cortical process which would account for the constant gray.

An early example of the same kind of *a priori* logic is to be found in Grassmann's establishment of Newton's law of complementaries in 1853. Newton had formulated the law (1704), but he was not sure that it held so exactly that only two complementaries, mixed in the right proportion, would give gray. They seemed in practice to yield 'a faint anonymous color' near gray, and Newton wondered whether a 'pure' gray might not require more than two colors in the mixture. Helmholtz (1852) tried superposing two square spectra at right angles, thus giving an illuminated square in which every possible paired mixture of monochromatic lights was represented at some point. The locus of complementaries in such a diagram ought to appear as two curves of gray, but Helmholtz could be certain only of the two blue-and-yellow points. Then Grassmann, a mathematician, laid down the principle that a continuous change of stimulus must give rise to a continuous change of sensation, that stimulus intermediates must give rise to sensory intermediates, and thus that there must be for every color one other color which, when mixed with the first in the right proportions, will give gray, because poorly saturated colors on all sides of gray can be got by paired mixtures, and this axiom of correlated continuities thus fixes the locus of an intermediate mixture which will give an exact gray. This is a case where logic was accepted by everyone, including Helmholtz (1855), when observation had proved inadequate. Of course Grassmann's isomorphism lay between sensation and stimulus, Müller's between sensation and physiological process. See H. Grassmann, Zur Theorie der Farbenmischung, *Ann. Phys. Chem.*, 1853, 3rd ser., 89, 69–84; Eng. trans. in *Phil. Mag.*, 1854, 4th ser., 7, 254–264.

late of isomorphism, according to his own statement, arose out of his desire to modernize Müller's axioms,[5] just as this note is motivated by my faith that it is now possible and wise to try to modernize Köhler by going back to Müller and beginning again.

The point about Müller's axioms is that they still seem sensible to a positivist[6] like myself, and that fact must mean either that there has been a failure of integrity of thinking at some point or that what was good parallelism in 1896 is subject to operational translation into good physiology in 1941. There is, of course, no need for everyone to make this translation. Positivists have too often taken the police role, asking to see the operational licenses of all concepts. The burden of translation, if there be any burden at all, lies with those positivists who feel insecure without operational reductions. Whether their modes of thought represent a step in the 'modernization' of science the future will decide. In the meantime, let us see whether the seeming validity of Müller's axioms means that they can be given this physiological translation, and whether any criticism of them arises in the undertaking.

Müller's first axiom and my translation of it are as follows:

Müller's Axiom	*Operational Reduction*
1. The ground of every state of consciousness is a material process, a psychophysical process so-called, to whose occurrence the presence of the conscious state is joined.	1. A subject who asserts the occurrence of a specific 'experience' actually is indicating the occurrence of a specific neural event antecedent to the assertion.

There is, of course, a basic difference underlying Müller's and my axioms. Müller is speaking of a symmetrical

[5] W. Köhler, *Gestalt Psychology*, 1929, 58–67.

[6] I am using the words *positivism* and *operationism* (and their derivatives) in the sense that I understand the *positivism* to which Köhler and Koffka object in psychology, as signifying an operational reduction of phenomenal data to other data which are not distinguishable from what have been called physical data. Cf. K. Koffka, *Principles of Gestalt Psychology*, 1935, esp. 684f.; W. Köhler, *The Place of Value in a World of Facts*, 1938, esp. vii. In this sense I am by temperament a positivist, a reductionist. Carnap's logical positivism is something more specific.

correlation of coördinate terms. I am considering a temporal disjunction. An 'assertion' is an effector-event, a statement, a public report, and is thus a consequent of the neural processes which its nature implies. There can be, says this axiom, no assertion of differentiation except there has been prior neural differentiation, though the converse is not true, since there may be neural differentiation which cannot lead to a report of difference.[7] I have to assume veracity and fidelity in introspection, but then Müller too had to make those assumptions. I am also, like Müller, omitting consideration of the *Aufgabe* in order to simplify the discussion, but it might be noted here that a common case of unreportable neural differentiation occurs when the *Aufgabe* is directed to some other kind of report, as when a subject cannot report intensitive differences because he was prepared observationally to indicate only differences of quality. Nevertheless my statement still matches closely one of Köhler's.[8]

One has to choose, of course, his own operational definition of consciousness, and I have chosen 'assertion' for the reason that I think it matches most nearly the introspective operation that Müller had in mind, and that the Gestalt psychologists have in mind when they are speaking of isomorphism. It would be just as easy to write an alternative set of definitions based upon discrimination as the operation for getting at consciousness. Fundamentally there is no difference between the two sets, for an assertion is discriminative in so far as it is specific or adequate.

The second axiom, split in half, with Müller on the left and me on the right, is as follows:

[7] Cf. esp. E. G. Boring, A psychological function is the relation of successive differentiations of events in the organism, PSYCHOL. REV., 1937, 44, 445–461; also Psychophysiological systems and isomorphic relations, *ibid.*, 1936, 43, 565–587.

[8] Köhler, *Gestalt Psychology*, 1929, 67: "*If, to me, my language is an adequate 'symbol' for my own direct experience, it is an objective symbol for those physiological processes at the same time.* . . . It does not matter very much whether I take it as a symbol for one or the other, since in the respects in which they correspond, there is no difference between them."

2a. To an equality, similarity or difference in the constitution of sensations . . . there corresponds an equality, similarity or difference in the constitution of the psychophysical process, and conversely.

2a. A subject cannot assert that there is a difference between 'sensations' unless there has been a difference in neural events antecedent to the assertion, nor a similarity of 'sensations' unless there has been some degree of functional equivalence among the antecedent neural events; but the converse is not true, because not all neural differences can give rise to an assertion of difference.

2b. Moreover, to a greater or lesser similarity of sensations there also corresponds respectively a greater or lesser similarity of the psychophysical processes, and conversely.

2b. Moreover, an assertion of greater or lesser similarity indicates a greater or lesser functional equivalence among antecedent neural events, and an assertion of identity of 'sensations' indicates the equivalence of the neural events in giving rise to that assertion; and conversely.

The reason that the operational *2a* is not reversible, like Müller's *2a*, lies in the fact that there can be no differentiation in report without antecedent differentiation in the neural processes upon which report is dependent, although not all neural differentiation can be sustained in the report. If it be argued that the situation is really symmetrical in that both the antecedent neural process and the consequent report have features irrelevant to the total process of reporting, no harm is done, provided the essentially directional nature of reporting is kept clear; but I have chosen *assertion* as a co-relative term which excludes all that is irrelevant to its antecedents. For instance, the assertion remains the same whether it be written or spoken.

Similarity I have translated as *functional equivalence* because I cannot conceive of any other good meaning for similarity in this context. Two neural processes would be similar in their relation to report to the degree in which they are interchangeable with respect to the same report, but I should offer no objection to a translation in terms of differentiation: "differentiation between reports is not greater than the differentiation between the neural events upon which they depend."

The third axiom becomes, with Müller on the left and me on the right:

3*a*. If the changes through which a sensation passes have the same direction, or if the differences which exist between series of given sensations are of like direction, then the changes through which the psychophysical process passes or the differences of the given psychophysical processes have like direction.	3*a*. If the direction of two 'sensory' changes is asserted to have been the same, then the two changes in the antecedent neural events must have been alike in some respect, since they are equivalent in giving rise to the assertion.
3*b*. Moreover, if a sensation is variable in *n* directions, then the psychophysical process lying at the basis of it must also be variable in *n* directions, and conversely.	3*b*. Moreover, if a 'sensation' is reported as variable at different times in *n* different ways, then there must have been *n* different modes of variation of the antecedent neural events; but the converse is not true, for different modes of neural variation may be equivalent in giving rise to the same assertion.

Axiom 3*a* applies to the single attributive dimension, like the pitch of tones. Such a dimension is continuous because different distances on it can be equated: one pitch difference can be made equal to another pitch difference in another part

of the scale. In other words, two neural differentiations are equivalent for the same report, but there is no necessity for their being alike in any other way. It is not impossible, as some of Wever's work seems to suggest,[9] that difference in neural frequency should be the condition for a report of the difference between low pitches, and a difference in particular fibers excited the condition for a report of difference between high pitches; yet, if such a situation were to exist, it would mean that a frequency difference had become equivalent to a place difference since the two differences would be functionally equivalent for report. Ordinarily, of course, we expect neural similarity for identical reports; we expect that the antecedents of visual extension must have a great deal in common with the antecedents of tonal volume, because the observer, knowing first about visual size, can immediately begin to pass judgment upon tonal size. There is, however, no necessity for such an inference, and an equivalence could be learned.

Axiom 3*b* takes care of different attributes, like the pitch, loudness, volume and density of tones.[10] Since these four attributes are distinct, there must, at the very least, be four modes of neural variation to account for them. You could not get Stevens' isophonic contours for them unless there were at least four neural continua in each of which different distances are functionally equivalent for report, and in no two of which differences equivalent for report are to be found. (This statement has nothing to do with the question as to whether an attributive continuum depends upon the innate properties of the nervous system or is acquired by learning,[11] as some have thought tonal volume might be, nor with the fact that the four continua are interrelated so that any values

[9] E. G. Wever, The physiology of hearing: the nature of the response in the cochlea, *Physiol. Rev.*, 1933, 13, 400–425, esp. 422–424; Wever and C. W. Bray, The perception of low tones and the resonance-volley theory, *J. Psychol.*, 1937, 3, 101–114, esp. 108–110.

[10] S. S. Stevens and H. Davis, *Hearing*, 1938, 70–75, 123–127, 160–166.

[11] On the contention that there is theoretically no limit to the possible number of attributes and that the existence of an attributive dimension does not indicate whether it is native to the organism or acquired by learning, see E. G. Boring, The relation of the attributes of sensation to the dimensions of the stimulus, *Phil. Sci.*, 1935, 2, 236–245.

of two ought to predict the values of the other two.) Axiom 3*b* is not reversible for the reason discussed under axiom 3*a*.

That is all. Is there a moral? Perhaps. One sees here why psychoneural isomorphism as an axiom is so convincing. The principle is based on the simple assumption that one can not talk (or make an assertion) without a nervous system whose functioning is antecedent to the talking. One sees too, I suggest, that no great pother need be stirred up because of the rejection of positivism by the Gestalt psychologists. They do not have to be positivistic if they do not want to be, and they can be translated when there is need. The process is bound, nevertheless, to be clarifying and is sometimes corrective, as when a *conversely* of Müller's turns out not to be true, but in general any clear dualistic statement implies the operations upon which it depends, and he who reads may work the operations out and then rest secure with whatever degree of specification he has thus achieved.

[MS. received January 17, 1941]

SOCIAL ORGANIZATION IN INSECTS, AS RELATED TO INDIVIDUAL FUNCTION[1]

BY T. C. SCHNEIRLA

New York University

I. Introduction

It is now a truism to say that any social organization represents a qualitatively new emergent level not equivalent to that which might be attained through a mere summation of the properties of its constituent individuals. Little difficulty is involved in justifying the description of a social group as a 'creative synthesis' of individual rôles. But trouble arises when attempts are made to give the statement further meaning by relating the group pattern to intrinsic factors centering around 'individual nature.' How does group organization arise in the given animal form, and what is its relation to individual function?

The concept of group as 'superorganism,' as developed by Wheeler (**44**, **46**) for the insect colony, represents one outstanding attempt to clarify the confusion. This approach was founded upon the striking analogies which exist between the functions of a metazoön animal conceived as a colony of cell-units and an insect social group regarded as an aggregation of unit-individuals. But what is to determine whether this concept is to be given a vitalistic meaning along the lines of the superentelechy implicit in Maeterlinck's (**25**) "spirit of the hive," or a naturalistic meaning such as that held by Emerson (**8**)? We believe that the outcome should depend upon the results of systematic investigations of interindividual relationships in representative group situations.

From this standpoint we propose to explore the relationship between individual nature and social organization in insects, first rather broadly, then in terms of interindividual relationships in a special collective function.

[1] Presented at the 1940 meeting of the Topological Society, at Smith College.

A realistic view of these problems is encouraged by a survey of the literature dealing with the individual insect's rôle as recipient and as contributor in general colony life. As Wheeler (**45, 46**) adequately demonstrated in his comprehensive theoretical treatment of the social problem for insects, the basic colony functions represent highly dynamic processes, —processes grossly misrepresented by any static doctrine of fixed heritable mechanisms. A variety of evidence abundantly confirming this view will be found in the works of Emerson (**7, 8, 9**), Escherich (**10**), Forel (**14**), von Frisch (**15**), Nielsen (**27**), Rabaud (**28**), Verlaine (**41**),[2] and Wheeler (**43, 46**), in particular. These writers have presented in various complementary ways the complex picture of normal colony life in the different social insects.

Any attempt at a summary of the general evidence on the social insects is out of the question here, although a systematic resumé of the known facts would be highly desirable as a preliminary. But in the first section, although dealing with the general colony situation, we shall have to confine ourselves to certain psychologically important aspects which have not received sufficient consideration. The necessity of restricting this discussion to the organic setting of the group should not exclude from view the complex set of environmental forces which intimately affects the life processes of the colony during its growth and decline (**1, 9**).

II. Early Socialization of the Individual

The ontogenetic aspects of the social problem have been underemphasized, we believe, although for a long time it has been recognized that new individuals developing in a colony of social insects must pass through a kind of initiation process. Büchner, in 1876, had this to say about the apparent state of affairs in an ant colony:

> The pupæ, entirely freed from their cases, and now young ants, are however not yet ready and armed with all the advantages of a true ant—as they ought to be if the instinct-theory were true—but still need the help and guidance of their elder sisters. As already

[2] See also *Bull. Soc. roy. Sci. Liége*, and *Mém. Soc. roy. Sci. Liége*, 1932–1936.

mentioned, these at first give it food, and begin to lead it about the nest, and train it in a knowledge of domestic duties, especially in the care of the larvae (3, p. 67).

There is little question that an early process of individual social adjustment actually occurs on some basis, but the fact and theory of the matter demand far more investigation that they have received.

Evidence concerning the incorporation of the individual insect into the colony, and the relationships of individual and group, has been formulated comprehensively by Wheeler in terms of his concept of 'trophallaxis' (44). This concept represents a system of causal relations in social adjustment based upon the predominant excitatory and attractive effect of food or of equivalent chemical or erogenous tactual stimulation. At the present time it provides our most adequate theoretical basis for working out an integrated solution of insect social organization. For various ants the process of socialization seems to begin even when the individual is a larva and to extend at least through the first few post-hatching days of the callow worker. Observational studies by Heyde (19) disclose significant changes in behavior during the early period of a few days just after hatching, behavior changes which are delayed but not eliminated when callows are hatched artificially into groups containing no experienced workers. Heyde reported the modification of what are at first crude and limited feeding responses into controlled and specialized adjustments of greatly enlarged scope: special ways of obtaining food from other individuals by presenting antennal-contact stimulation, susceptibility for food-delivery when stimulated by other individuals or by the presence of brood, and finally an expansion into foraging outside the nest. The social space of the worker evidently enlarges greatly during her first post-hatching days, through the elaboration and modification of limited initial responses, until it includes all properties of the colony situation and extends considerably beyond the nest environment.

The nature of this developmental process is very incompletely known at present. Certainly it is inadvisable to

confine one's theory to either a preformistic or an epigenetic cast, since factors relevant to both are involved. For example, Heyde (19) conceptualized the worker ant's earliest responses (mouth-part and general anterior-end activity, elicitable tactually and chemically) as 'reflex,' and later activities (*e.g.*, the callow's approach to other individuals with excited antennal oscillations) as 'instinctive.' But such interpretations undoubtedly misrepresent the process underlying the behavioral modification. For, in addition to the native factors unquestionably involved, there are excellent reasons for postulating a gradually established generalized learning as an essential dynamic factor in the insect's social development.

This is not a new idea. First suggested by Walsh in 1864, it was given new emphasis a few years ago by Thorpe (36) and by Uvarov (40), and was employed as a postulate in the general theory of insect social behavior advanced by Maier and Schneirla (26). Just recently Thorpe (37, 38, 39) has furnished a convincing experimental demonstration of its validity, finding that an ordinarily ineffective chemical which is present in the environment during the period of larval feeding may become a controlling factor in the feeding or other specialized responses of the adult. Thus the ichneumon fly *Nemeritis*, which normally responds with its egg-laying thrusts to larvæ of the flour-moth *Ephestia* but not to larvæ of the bee-wax moth *Meliphora*, exhibits this specialized response to the latter insect if raised upon it during the larval feeding period.

In a similar manner group dependence may become primarily ingrained in the individual social insect through a kind of conditioning process based upon the constant presence of the pervasive species-colony chemical during the highly reinforcing vegetative condition of feeding. Such an occurrence is strongly suggested by experiments with mixed-colonies of ants, in which ants of different species are placed together from the time of hatching, in isolation from others of their respective own species (12). These experiments show that subjecting the individual from the time of hatching to the

chemical environment of a different species, or to a compound of species chemicals, results in a regular colony adjustment to individuals bearing the habituated chemical but a 'strange' or inimical response when placed with her 'own' species (**2, 11**).

Superimposed upon this chemically-determined group habituation are tactual mechanisms which further expedite relationships of interdependence among individuals and sections of the colony. The brief antennal exchanges when worker meets worker are not expressions of a 'code,' qualitatively distinctive symbols conveying social information, as Wasmann (**42**) maintained and popular writers are fond of saying. They are a learned modification of the larval feeding response, as Heyde's (**19**) findings indicate. Careful investigations have shown that the antennal taps function as sources of a generalized excitatory stimulation, *i.e.* as 'releasers,' to use Lorenz's (**24**) term, eliciting socialized actions of a more or less appropriate nature in dependence upon individual condition and prevailing environmental circumstances.[3]

III. Organic Factors in Colony Integration

Processes of social adjustment such as we have been describing develop under the influence of a set of guiding and limiting factors imposed upon them by organic maturation. The recent investigations of Goetsch (**17**) in particular show the manner in which social functions are differentially assumed in close correspondence with the morphological characteristics (*e.g.*, general size, head structure, mandibular structure) of respective worker castes. Chen (**4**) has de-

[3] We may briefly mention some of the evidence which strongly opposes an anthropomorphic interpretation of insect communication. First of all, the careful investigations of von Frisch (**15**) and his collaborators (**16**) definitely show that the activity of the excited 'finder' bee does not of itself *specifically* excite other bees,—no definite kind of response follows the hive-arousal unless other bees have learned their own way to respective food-places, or unless a chemical is involved to which the excited bees specifically respond. The evidence for the investigated ants follows corresponding lines. Santschi (**31**) has shown for a *Tapinoma* species that the differentiating factor is a secretion from the anal-gland of the excited 'finder' ant, hit upon and followed by others when antennal tapping bestirs them; Eidmann (**5**) reports very similar results for a *Myrmica* species, with careful control experiments; and Schneirla (**26**, p. 170) reports that in various *Formica* species the differentiation of response in secondarily aroused workers may depend upon what foraging routes have been learned by different individuals.

scribed the close relationship which prevails between organic mechanisms and environmental conditions in the determination of nest-building activity.

Very significant in this connection are studies which show that changes in social function are initiated as consequences of successive changes in organic constitution during the life cycle of the same individual. Thus, Rösch (**29**) found that young bees turn from 'brood tending' to 'building' when the active phase of their pharyngeal glands is replaced by that of their wax glands, and that toward the end of their first three weeks when the latter structures decrease in function the bees change to foraging in the field.[4]

IV. The Brood as Source of Social Stimulation

Throughout the social insects the individual change of greatest significance for social processes is that from the larval form (in which the individual is a voraciously feeding 'grub') to the adult insect form with its greater motility and behavior specialization. The social contributions and interrelationships of the adult insect in their general nature are matters of common knowledge. Those of the larva, although less specific and much less directly effective as influences upon group function, are also highly important for the social structure. Thus Wheeler (**46**, Chap. IX) emphasized as one important phase of trophallaxis the larval exudates and other nutritive and chemical effects emanating from the young which provide a basis for establishing and continuing adult responses to the brood.

A striking example of brood trophallaxis as an integral social factor is found in the American doryline ants, the Eciton species studied by Schneirla (**32**, **34**, **35**). In the rainy season new broods of these ants (numbering many thousands of individuals) appear at regular intervals of about

[4] A sharper emphasis upon the rôle of the organic substrate arises in cases which involve a considerable change in structure and in function during the same phase of individual life history. Such cases are not common among insects. A striking instance was reported by Emerson (**6**) concerning a tropical termite, in which a single molting found some of the nymphs changed both morphologically and functionally in their colony behavior from the 'worker' to the 'soldier' form.

35 days, and since the eggs of a given brood are laid within a short period of a few days, the individuals develop concurrently. While each brood is passing through the main part of its larval stage (a period lasting about 18 days in the two species studied) in which the larvæ are ravenous and very active, there is a notable change in colony activity. Throughout this period the daily raiding foray of the colony attains a height of activity, a maximum in number of participating individuals, and a broad limit of development both spatially and qualitatively in its pattern, not attained at other times. Also, throughout this period each daily raid terminates in a migration of the entire colony to a new nesting site in the periphery of the day's raiding zone. At other times (*i.e.*, throughout the period in which the brood passes through the pupal stage enclosed in cocoons) the daily raids are minimal in development and there is no migration.

These and other facts suggest a causal relationship between brood condition and general colony behavior. Schneirla has concluded that the larval brood through its activity furnishes a 'social stimulation' which greatly increases the level of general colony excitement, thus indirectly accounting for the change in the pattern of colony behavior (**34**). More specifically, tactual and chemical stimulation from thousands of squirming larvæ excites the workers to a much higher degree and propagates this effect more widely through the population than can be effective at other times when (with the brood enclosed in cocoons) workers alone are the source of social stimulation.[5] At such times the spatial range (and hence the 'social space') of each given adult individual is greatly increased, for in her highly aroused condition she roams about more widely within the nest area. This activity permits her to stimulate greater numbers of nest-mates within a given time than may her more restricted activities when the larval-trophallaxis factor is absent.

[5] It should be pointed out that the raid first begins and thus is 'timed' through the effect of an extrinsic factor, stimulation of the colony by the first light at dawn (**34, 35**). The point of this discussion concerns the fact that the condition of the colony at the time determines the amount of intrinsic reinforcement which the first response receives, and hence limits the development of the raid.

With the 'social space' of each worker greatly enlarged as an indirect but inevitable result of the brood-trophallaxis factor, larger numbers of workers are 'expelled' from the bivouac than when this factor is absent. The maximal raiding thus attained, a qualitatively distinct mark of colony behavior throughout this period, greatly enlarges the 'behavior space' of the colony on each given day. Also, since the raiding inevitably passes over into a migration at each day's end, fresh booty-containing terrain is always available. This of course greatly increases the total 'colony space' of the period. The adaptive significance of these facts, indirectly effected through brood-trophallaxis, is self evident.[6]

V. Some Relationships in Foraging

To consider the process involved in the foraging and food-delivery of the socialized worker herself, we may turn to an ant genus such as *Formica* in which individualized foraging is common. As we have said before, the act of foraging outside the nest constitutes a specialized extension of the simple food-delivery process developed in the worker during her early period within the nest confines. The heritable basis of this ultimately complex socialized process is a reflex mechanism by which food is pumped back from the crop upon adequate stimulation of mouth-parts. The nature of this process literally obligates any worker to deliver nutriment upon 'call.'[7] We have pointed out that the

[6] There is evidence that in some insects the presence of a brood is directly essential for the viability of the colony. For instance, various investigators have experienced failure in their attempts to transplant colonies of the giant *Paraponera clavata* and related ponerine ants from the tropics to the temperate zone for laboratory study. But Zahl (47, pp. 175–178) solved the problem when he accidentally introduced some larvæ into a group of dying workers. These isolated adults promptly responded to the larvæ, licking and handling them, and soon recovered their health. Perhaps a related instance is my finding (for the dorylines) that *Eciton hamatum* queens die within a week if isolated from workers and brood, but otherwise survive. Such indications of the vital importance of trophallaxis in the 'primitive' ant subfamilies should be explored in relation to the general problem of population growth and decline (1, 9) as well as the problem of group organization.

[7] It is significant that no digestion occurs in the highly distensible crop, from which food may be regurgitated: hence Forel's term '*social stomach*' (14) accurately describes the function of that portion of the digestive tract. The crop is separated from the *individual stomach* posterior to it by a tube (the proventriculus) which functions as a valve. The pumping action whereby fluid is regurgitated may depend upon the action

functioning of this entire apparatus is evidently expanded and specialized into wide social relationships through an early generalized learning process.

Among numerous modifications in this response as the worker is assimilated into the group, one change in particular is of great importance for the apportionment of food. In many ant species, the replete forager upon returning to the nest soon breaks away from each separate food-delivery act after a few seconds, and abruptly runs onward. Similar brief acts of delivery to other individuals ensue successively before the load has been discharged. In the early colony life of the worker, as Heyde (19) reported, the adequately stimulated callow regurgitates at length once the process has begun. But within the first days of foraging the reflex delivery process becomes progressively limited to shorter intervals of action, thereby minimizing the amount of food which any individual may receive at one time. Hypothetically this inhibitory process may be attributed to the dominance of a learned foraging-set (running-with-food) which can be displaced only momentarily when the worker is laden.[8] This is only one example of the manner in which the early socialization process extends the relationships of interdependence among individuals.

VI. Group Organization in a Special Function

The bulk of our knowledge about the social organization of insects has been obtained from observations of activities centering around the nest site or of other functions involving

of muscular parts of the proventriculus (13) or upon peristaltic action of the pharynx (21), or both. It is important to note that both of these muscular portions of the digestive tract are innervated by nerves of the sympathetic system which reach them from the frontal ganglion rostral to the brain (20, 12, 43).

[8] In the replete worker evidently an internal stimulation is provided which maintains the postulated running-set, or returns it promptly to dominance. If this holds, we should expect that as the contents of an ant's crop become lightened through regurgitation, further separate delivery acts would terminate with decreasing readiness, since presumably the inhibitory internal stimulation initially in effect would progressively fall off. This should be tested. The hypothesis seems to find indirect support in Kalmus' (22) finding that stimulation from a fully charged crop accounts for the cessation of a feeding act in the honey bee. The feeding intervals, normally predictable and limited in length, became very long and highly variable when puncturing of the crop precluded attaining a condition of repletion.

the performance of relatively small numbers of individuals within a limited behavior field. It is very difficult to arrive at an evaluation and analysis of organized activities involving the collective efforts of a large section of a colony population, and such work has progressed very slowly. For one thing, the various phases of the aggregate function seldom lie within the perceptual or experimental control of an investigator at one time. It is desirable to find and study aggregate performances in which the observer is not subject to this limitation.

Emerson (7) has met the difficulty in an ingenious way by studying group integration in terms of the final outcome of collective labors: the nest-pattern. In this way, the scope of social organization in many termite groups has been evaluated cumulatively by comparing the architectural character of the various edifices produced. The nests are thus regarded as "morphological indications of behavior patterns . . . [which] express the behavior of a population." By this method light is cast not only upon the properties and limitations of contemporary group integration, but also indirectly upon the phylogenetic background of species variations in the central behavior pattern.

Another approach is to study the collective behavior of a large group of individuals engaged in a special function at a given time. The remainder of this paper will be devoted to the analysis of such a group behavior phenomenon investigated in the field (34).

VII. Analysis of Organization in the Eciton Swarm Raid

In the 'primitive' ant subfamily *Dorylinæ* collective foraging outside the nest site commonly attains massive proportions and a high degree of organizational development. This is particularly true of the swarm-raids carried out by the large terrestrial species *Eciton burchelli*, which the writer has termed ". . . definitely the most complex instance of organized mass behavior occurring regularly outside the home site in any insect, or for that matter, in any infra-mammalian animal" (35, p. 401). Not only is it possible to

investigate the functional pattern of such groups under given conditions, but the development of the phenomenon may be studied with the same colony in successive new environmental situations.

The representative pattern of a swarm raid and its relation to the colony bivouac are sketched in Fig. 1-*A*. The swarm, a massive body moving broadside away from the bivouac, is connected with the home site by a fan-shaped mass of columns just behind it and by a single column farther in the

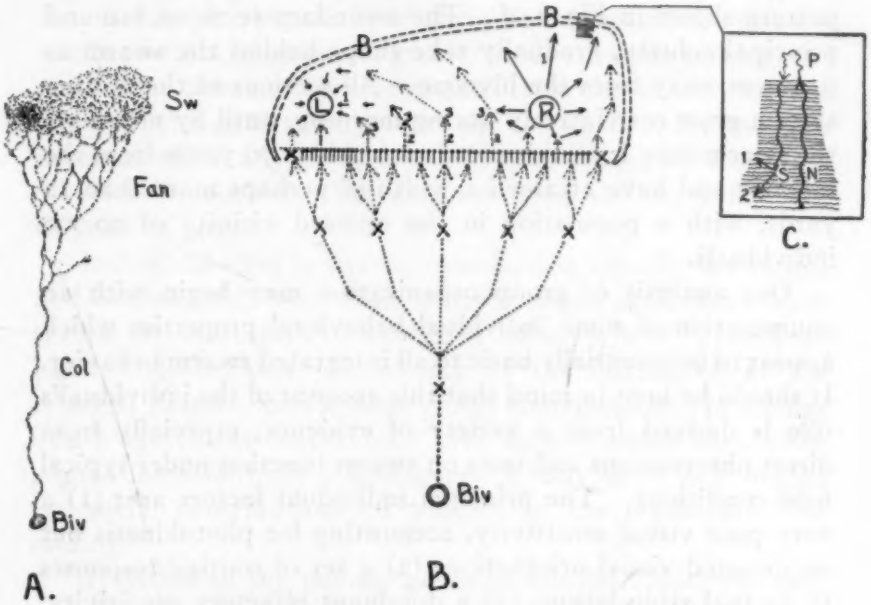

FIG. 1. *A*. Simplified sketch of an *Eciton burchelli* raid in the early hours of activity. *Biv*, bivouac (colony cluster); *Col*, principal column; *Fan*, network of columns in rear of main raiding body; *Sw*, swarm (8 to 15 yards in width).

B. Schematized version of the system represented in 1-*A*. *Biv*, bivouac; *X-X*, *basal pressure* exerted upon swarm by exodus from bivouac; *B*, *frontal barrier* of the swarm; *L*, left flank, undergoing a concentration; *R*, right flank, undergoing an expansion; 1, solid arrows at *L* and *R* indicate principal directions of change; 2, lines of basal-pressure effect as propagated forward through swarm; 3, see text.

C. Schema of the rebound reaction of an individual ant at anterior border of swarm (representing a small area of Fig. 1-*B*). *N*, *normal course* of the *Eciton* across chemically-saturated terrain; *P*, *pioneer phase* (hesitancy and recoil) when unsaturated terrain is entered; *S*, point on return at which contact with ants in swarm becomes effective; 1, a reversal of progress through intense or summated contact; 2, a limited deflection elicited by weak contact.

rear. A consideration of typical morning events, which are fairly regular and predictable, will satisfy our present purposes.

A predictable sequence of changes occurs in the pattern of the raiding system and in swarm behavior, as new recruits steadily increase the population of the raid during the morning period. The initial event is an expansion of ants radially in all directions from the bivouac. Then one or another section of this mass becomes increasingly dominant, takes on unitary characteristics and absorbs adjacent sections, and increases the directionalization of its progress until it assumes the pattern shown in Fig. 1-*A*. The secondary sections, fan and principal column, gradually take shape behind the swarm as it moves away from the bivouac. All sections of the raiding system grow correlatively during morning, until by noontime the swarm may have advanced as much as 70 yards from the bivouac and have attained a width of perhaps more than 12 yards, with a population in the upward vicinity of 20,000 individuals.

Our analysis of group organization may begin with an enumeration of some individual behavioral properties which appear to be essentially basic to all integrated swarm behavior. It should be kept in mind that this account of the individual's rôle is derived from a variety of evidence, especially from direct observations and tests on swarm function under typical field conditions. The principal individual factors are: (1) a very poor visual sensitivity, accounting for photokinesis but no directed visual orientation; (2) a set of routine responses to tactual stimulation; (3) a dominant olfactory sensitivity, accounting for a stereotyped following of the colony chemical; and (4) glandular functions accounting for the release of *Eciton* chemical, especially when the worker is very excited. The first factor, visual condition, is basic to the 'timing' of raids (*i.e.*, through morning arousal by light), but in the group-raiding situation it contributes only indirectly by offering no interference to the dominance of the other factors.

The latter three factors in combination establish a pattern of individual behavior which is implicated basically in all group function, and which may be termed the *rebound pattern*.

This individual pattern, which is sketched in Fig. 1-*C*, involves a normal course, and in the swarm situation two typical end-points of change. The *normal course* of the pattern concerns regular locomotion under facilitation by *Eciton* chemical, as in the direct following of a chemical trail. One end-point, the *pioneer phase* of change concerns the typical reversal of progress when chemically unsaturated terrain is encountered (and 'social' facilitation is thereby removed). The second end-point, which we may term the *swarm phase*, concerns changes elicited by tactual stimulation in the swarm. Intense contact, especially if abruptly encountered, typically elicits reversal of progress; gentler contact elicits turning and movement toward that side or facilitates continued progress. The rebound pattern represents the essentials of the worker's involvement in any group activity, with the partial exception of behavior such as that characterizing the capturing and disposal of prey.

Individual rôles in swarm organization actually are simple and stereotyped, with their differentiation dependent upon special circumstances of collective behavior and of environment. All workers are subject to the conditions of the rebound pattern, hence their group contributions are similar under equivalent conditions.[9] We may consider the *Eciton* population essentially a homogeneous body as far as concerns the social contributions of workers in the organization of raiding.

By virtue of the pioneer phase of the individual rebound pattern, new terrain serves as a barrier to an advancing group of *Ecitons*. In figure 1-*B* the composite behavior effect of the chemically unsaturated ground in the path of an *Eciton* swarm is represented as the *frontal barrier*. But the *normal course* of the individual reaction usually has a limited perse-

[9] So far as group integration is concerned, this statement holds even for the extreme individual forms in the polymorphic series, the workers major and workers minor. However, such individuals differ sufficiently in size and motility from the intermediate workers to account for *secondary* departures from the normal action pattern in their case (35, note 16, p. 449). These reservations are relatively inconsequential for our discussion, since the great bulk of the swarm population consists of intermediate workers.

verance, and the worker advances hesitantly a few centimeters beyond the boundary of the swarm-saturated zone before turning back. Highly excited, she saturates this short line of new terrain with a glandular product which facilitates the advance of the next comers, and thus slightly extends the anterior boundary of the swarm. Hence the frontal barrier may be said to offer a yielding impedance to the swarm's advance. Because of the regularity with which unconsolidated ground enforces the retreat of pioneers into the swarm, this impedance operating at the anterior boundary has a constant reality for swarm behavior. To understand why a similar restriction of the lateral swarm boundaries operates in a far more rigid manner, other major factors must be considered.

It must be emphasized that coping with the frontal barrier is a *group* function. In dependence upon the circumstances of stimulation encountered when she rebounds from the barrier into the swarm space, the worker may be caused to repeat the pioneering act, may become involved in a local predatory activity, or may be diverted into an intersectional movement within the swarm. Clearly, 'leadership' of the advance is an aggregate function into which a plurality of individuals near the anterior boundary are certain to be carried regardless of their activities in the just preceding moments.

The persistent advance of the swarm against the fluid frontal barrier is maintained by a behavioral factor which may be termed *basal pressure*, generated through the arrival of a fairly continuous stream of recruits from the bivouac. Pushing up at the rear of the swarm, these ants furnish a composite tactual stimulation which directs rebound reactions in adjacent swarm groups preponderantly away from the bivouac. The dotted arrows ('2') in Fig. 1-*B* indicate that a general moment of force is thereby created by the behavioral propagation of this pressure effect forward through the swarm. Because this behavioral factor in the long run exerts its effect upon all sections of the mass, and equivalently upon the two wings, it maintains a general advance of the swarm away from the bivouac.

The *basal pressure* effect propagated forward through the swarm persistently works against the *frontal barrier*, and dominates sufficiently to insure a fairly steady locomotion of the mass into new territory. The conflict between these forces expresses itself rather directly in the reinforcement of lateral component movements which have a commissural significance for the organization of the swarm. Another outcome is a bilateral extension of the mass as it grows, a flattening at right angles to the line of general advance which marks the typical form of the body (see Fig. 1-*A*).[10] But since the general moment of basal pressure is 'forward,' the lateral swarm boundaries represent barriers which are overcome only very slowly through this laterad extension of the mass. As barriers they are effectively 'rigid,' as compared with the 'yielding' frontal barrier.

The dominant effect of basal pressure is variably opposed and altered by the inertia of local groups attacking prey, or by major topographical features which canalize or otherwise influence the movement of local sections of the main body. This at times accounts for marked distortions in the movement and pattern of the swarm and for delays in its advance (**35**, pp. 435–439).

Organization in the swarm is effected through the interdependence of its variously active sections. Basal pressure is a surrogate factor in the total process, since its effects are operative more or less persistently throughout the entire mass. Its broad dominance is revealed by the manner in which the swarm loses headway and direction, and basically changes in its organization, when this pressure factor is displaced either naturally or experimentally. Such an interruption regularly occurs in the latter part of the morning, when a stream of returning booty-carriers directly opposes the exodus of recruits from the bivouac, and first disperses the effect of basal pressure in the fan and along the principal trail, and later cuts it off at the source. Then the swarm gradually loses its

[10] It is a significant fact that the persistent morning exodus from the bivouac which regularly augments the raiding population does not accelerate the rate of swarm advance to a noticeable extent. Instead, the most apparent result is a gradual widening of the swarm throughout the morning period (**35**, pp. 415–417)

unitary organization and undergoes schizmogenesis. This presently eventuates in an actual fission of the mass, producing two or more new sub-swarms which thereafter function as essentially independent groups (**35**, pp. 434–435).

We may postulate two fairly distinct *transmission agencies* as essential intrinsic factors in swarm integration. One is *pressure;* the other may be termed *drainage.* Pressure transmission, already referred to, arises mainly from the proneness of the *Eciton* worker to turn away from the side of repeated or strong tactual stimulation. "Thus the entrance of newcomers into a group at one side first tactually elicits the rebound (retreat) reactions of nearby ants, these in turn affect others more remote from the original source, and so on, until the behavior-change may have become transmitted across the entire group" (**35**, p. 425).

Drainage transmission is essentially an outcome of the worker's proneness to turn toward the side of weak tactual stimulation and to continue running on *Eciton*-saturated ground. "When a section of the swarm shifts its position . . ., nearby workers in their incessant and variable movement within limited space soon enter the vacated area,—more promptly if brushed tactually by the departing workers. In this movement their progress is unimpeded, whereas variations taking them in other directions are curbed by repeated tactual stimuli occasioning rebounds. As groups successively farther from the originally vacated area become involved in this manner, the change in action is propagated through a large section of the swarm. Thus, in evacuating chemically saturated terrain, raiders literally 'drain' others through the area in the same general direction" (**35**, pp. 425–426).

More or less opposite in their nature and function, the behavior sequences represented here as 'pressure' and 'drainage' are found causally involved in all of the various intersectional relationships which weld the raiding body into a unit.

The integration of the swarm may be studied effectively in terms of direct spatial properties. The dominant sectional

groups of the swarm are established in its flank zones. These zones typically hold the nuclear sections because the circumstances of their position in the mass give each of them just *one* highly fluid behavior boundary, that on the central side. This is in contrast to the central zone itself, which has *two* highly fluid behavior boundaries and thus typically becomes a subordinate zone across which behavior-changes are transmitted between the flank sections. From Fig. 1-*B* it may be seen that each flank zone has three barriers: *environmental* barriers frontally and laterally, and a *behavior* barrier in the rear furnished by the dominant basal pressure. Hence in each of the wing zones, under the energizing influence of basal pressure, there is an opportunity for building up new behavioral conditions which may be modified but not readily dissipated by effects transmitted from other zones.

But the conditions which give the flank zones their unitary integrity as functional sections of the swarm can hold only exceptionally for the central zone. This section of the mass typically is held down to a commissural rôle through its susceptibility to behavior-changes imposed upon it simultaneously from both wings.

Analysis of the previously described flanking movements is the most effective means of representing swarm organization. In a given flanking movement, the nuclear events prove to be behavior-changes of approximately opposite nature concomitantly developed in the two wing sections of the swarm. While one wing (*e.g.*, '*L*' in Fig. 1-*B*) undergoes a concentration of its personnel, thus serving as pivot for a partial turn of the swarm, the other wing ('*R*,' Fig. 1-*B*) passes through an expansion directed mainly forward and obliquely inward.

This expansion of the temporarily mobile wing is checked fairly effectively by its lateral boundary, but is projected strongly forward by virtue of reinforcement from the basal pressure. Also, a pressure transmitted centrifugally from the local expansion itself maintains a shift of columns and masses of ants through the intermediate zone, across the central behavior boundary toward the opposite flank of the

swarm. This movement is facilitated by a drainage effect from the opposite (concentrating) flank, the transmission of which sets up a moment of behavioral attraction toward that wing through central zones.

The coincidence of the pressure and drainage effects in supporting a lateral drift of ants toward the concentrating flank illustrates how the transmission agencies coördinate the sections of the swarm in action. But as would be expected, this lateral component is given an oblique thrust forward through its interaction with the basal pressure ('3,' Fig. 1-*B*). The result is a general swing of the entire swarm through approximately 20 degrees obliquely forward toward the side of the concentrating flank.

A further episode in swarm organization is produced in the subsequent reversal of this flanking movement in a swing toward the opposite side. The former pivotal wing thus becomes the mobile part of the mass, and *vice versa*. Hence this new episode is marked by a reversal of the preceding local events in both wing sections. The change from concentration to expansion in the one flank is mainly an outcome of the centrifugally propagated pressure which soon becomes dominant once the previous concentration has reached a given maximum. On the opposite wing, transition of the preceding expansion into a concentration is based upon a centripetally directed drainage which inevitably comes into effect after a given maximal point of expansion is attained.

Thus the nuclear events involve rhythmic processes of change, but not primarily because of events in the wing sections themselves. It must be emphasized that both of the nuclear changes of the reversed flanking movement are energized by the constant action of basal pressure. This factor precludes the persistence of a condition of equilibrium in the function of either principal sub-section of the swarm, and thus enforces a regularity of organized change in behavior.

We have emphasized the fact that general group integration is effected through the interdependent relationship of the two principal sub-sections, by virtue of reciprocal behavior effects transmitted across the fluid central zone. The

immediate mechanism of swarm organization thus concerns intrinsic factors. However, the intrinsic agencies operate under the influence of a dominant energizing and directionalizing factor grounded in the population base. As we have mentioned, a natural control experiment on this matter is regularly carried through in the late morning, when homeward-bound booty-carriers begin to dominate the principal trail. The breakdown of swarm organization which then occurs, eventuating in a greatly altered total pattern, definitely reveals the normal surrogate function of the basal pressure factor.

VIII. Conclusion

It is clear that the pattern of group behavior exhibited at a given time by individuals in the army-ant swarm is the product of conditions both intrinsic and extrinsic in nature. Our analysis has shown that the characteristics of the *Eciton* individual, although decidedly limited and stereotyped in themselves, in the group situation may cumulatively result in a rather complexly organized and highly adaptive total function. 'Leadership' is a generalized property of the aggregation, since individual rôles—whether 'pioneering,' predatory, or 'transmission' activities are involved—are subject almost momentarily to change according to the circumstances of individual placement in the general situation.

Thus the properties of the individual eventuate in a highly complex development of group action, a result which appears emergently through a hierarchy of interindividual behavior relationships arising inevitably in given collective settings.

In the raiding situation, the cumulative function of the basic individual 'rebound pattern' contributes on the one hand to the transmission factors pressure and drainage; on the other hand it accounts for the existence of environmental and behavior barriers highly essential to the group behavior pattern. Basal pressure, one complex result of collective individual 'rebound' activities, is the driving factor in overcoming the environmental barrier. The patterning of pressure and drainage functions (under given barrier condi-

tions) gives rise to the nuclear group events (the integral flank activities) which serve as essential part-processes in the total swarm pattern. While the basal pressure is operative, the transmission agencies interact to coördinate the part-processes into an effective group organization.

Only by postulating intermediate functions such as these can the relationship between individual behavior and complex group behavior be understood. This conclusion from our study of events on a lower psychological level seems very compatible with the field approach and conceptualization as Lewin (**23**) has outlined it substantially for the mammalian level.

The two principal sections of this discussion have portrayed systems of events which through their interdependence provide a broad basis for the general pattern of group organization. There clearly exists a close relationship between the special group function of foraging which we have just described, and the organic properties of central colony life sketched at the outset. The most prominent outcome of trophallaxis is foraging,—*i.e.*, the instigating cause of foraging is a social stimulation which is a by-product of trophallactic relationships in the colony. In the *Eciton* colony when a larval brood is present this 'social stimulation' is evidently doubled or tripled in forcefulness, judging by the size, complexity, and spatial extension of the daily raiding forays during that period. Furthermore, the functional space of the colony is greatly amplified throughout such periods by the fact that the daily raids invariably reach the threshold of transition into migratory movements (**34**).

It is highly significant that an augmented social stimulation arising from a colony at the height of its food-consuming capacity, accounts indirectly for a larger food supply through its driving effect upon the development of raiding activities. This is also the case for the highly evolved *Formica* species of ants, whose foraging (*i.e.*, as concerns establishing a route and actually transporting food) is largely or often entirely a matter of individual activity (**33**). As we have said, in *Formica* life the direct effect of mechanisms insuring collective

behavior is manifested in the nest situation,—much less specifically in the environment outside the nest zone. But we feel justified in contending that in both the *Formica* and in the relatively primitive *Eciton* species the instigating social forces give rise to highly adaptive end results through comparable hierarchies of functional agencies basically grounded in the properties of the individual.

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THE METAPHOR OF 'CLOSURE'

BY N. SCHOENFELD
Columbia University

I

It is almost thirty years since Wertheimer's paper on apparent movement launched Gestalt psychology. In those years Gestalttheorie has erected a formidable structure and has proved itself to be an aggressive and fruitful sector of psychological research. It has done yeoman service in underscoring some old problems and restoring to them their true importance, in exposing new areas of hitherto unrecognized problems, and in developing a new and valuable orientation from which those problems can be approached. For these achievements Gestalttheorie deserves the praise and gratitude of all psychologists.

The rapid rise of Gestalt psychology has naturally been reflected in the psychological literature. The greater proportion by far of the writings addressed to Gestalttheorie has been protagonist in nature. The healthful criticism which is so necessary in science as a check upon the occasionally careless experimentation and unbridled conceptualizing of scientists is very meager in the literature dealing with Gestalttheorie. Yet there is no dearth of criticism in the private intercourse of psychologists. It appears to me that there are two general reasons for this curious reluctance.

The first reason is the difficulty of comprehending much of the writings of the Gestalt founders and formulators. It is not easy to obtain from them clear-cut and concrete statements of their assumptions, concepts and principles. Those psychologists who have ventured to speak some words of criticism usually find themselves informed that they did not *really* grasp the meaning of Gestalt psychology on the points criticized, and that Gestalttheorie adequately deals with them. Whereupon, the theory is immediately extended in the necessary direction.

The second reason is that the unusually rapid development of Gestalt psychology, coupled with the fact that it has such substantial contributions to its credit, has created an unwillingness on the part of many psychologists to put their critical reflections in the record. They are never sure that some ingenious Gestaltist will not, on the very morrow of publication, retort with a new 'proof' of the unassailability of the Gestalt position, and they know from experience how strangely incomprehensible a Gestaltist can make his words when he really puts his mind to it. Since incomprehensibility may be turned into an accusation of inability to comprehend, one does not like to invite Gestalt retorts. Furthermore, the seeming ease with which a Gestaltist resorts to physics has only served to heighten that wariness, for physics is a relatively unfamiliar and awesome realm to most psychologists. Even those with some knowledge of physics find it a frustrating task to counter arguments based on what Gestaltists consider to be physics.

For these reasons, then, one finds that the number of good critical papers on Gestalttheorie are too few; among the better ones may be mentioned a series of exchanges between Rignano (**21**, **22**) and Köhler (**12**, **13**), a provocative article by Lund (**16**), a broad attack by Eilks (**3**), a slashing assault by Spearman (**23**), a series of astute papers by McDougall (**18**, **19**, **20**) together with Koffka's rebuttal (**11**), a keen challenge by Wyatt (**27**).

I have made some rather outspoken remarks upon Gestalttheorie and Gestaltists in the preceding paragraphs, and it is the purpose of the present paper to substantiate them to some degree. The criticism I have to voice here does not deal with any of the complex principles or generalizations of Gestalttheorie, it is not an attack on its methodology nor on any course of Gestalt logic. I merely wish to take the term 'closure' as a sample from the Gestalt vocabulary and expose its present inadequacy as a concept on which we can build. There is no doubt that the term designates a valid problem for psychological investigation. Yet, I hope to show that this term, with which they make great play, has not been

adequately defined as yet and that it has been extended as an explanatory concept to a number of behavioral fields where its meaning is so attenuated as to be non-existent. In so doing, my main reliance will be upon the written words of Gestaltists themselves.

There is a certain equality of importance which Gestaltists apparently feel toward their basic terms like good Gestalt, pragnanz, and closure. Hence, I do not wish to imply that in taking 'closure' for criticism, I am selecting their most basic term. On the other hand, I feel that many other words in the Gestalt vocabulary may be, and deserve to be, similarly treated. Recurring examination of the use of terms is a fundamental requirement of all sciences, since no sound superstructure can be built upon uncertain pillars.

II

That the phenomenon of 'closure' occurs in perception, no one, I presume, will question. The trouble arises when one seeks to define the phenomenon in such a way that instances of it may be unfailingly recognized, or the occurrence of it as a consequence of certain definable operations invariably predicted.

The attempt to obtain a formal definition of 'closure' from the Gestalt literature yields little satisfaction. Koffka merely informs us that "closed areas are more stable and therefore more readily produced than unclosed ones" (10, p. 167). He gives numerous examples of 'closure' and develops some of its properties. This procedure, so commonly used by Gestaltists, merits a word. Any psychologist can give examples of perceptual 'closure,' since the phenomenon is intuitively or empirically understood to some degree by all of us. Elementary considerations of logic, however, inform us that formal scientific definition requires something more than mere exemplifications of a phenomenon. Unfortunately, the Gestaltists do not seem to feel any concern for the requirements of logical definition and blithely proceed with discussions and ramifications of their "closure principle."

Wertheimer's formulation of the "factor of closure" is a study in redundancy: When *A*, *B*, *C*, *D* are present and *AB*/*CD* yield two closed processes, and *AC*/*BD* two unclosed ones, then *AB*/*CD* are preferred (**24**, p. 325). The very terms 'closed' and 'unclosed' which we wish to have explained are blandly employed in the 'formulation' without any attempt at definition. Wyatt (**27**) pointed out a long time ago how prone Gestaltists are to commit the "fallacy of treating the problem to be solved as its own solution" (p. 309).

Köhler (**7**, p. 45) defined 'closure' as follows: "In every process which issues at all in an end-situation, independent of time (although it is possible to apply this to events occurring in time), the mode of distribution (of energy) shifts in the direction of a minimum of configurative energy." Here we have what seems to be a concrete statement of the 'closure principle.' However, any but a superficial view will disclose that we have here only one more evasion. What is a 'mode of distribution of energy' or 'minimum of configurative energy' in animals? Does he mean to have this definition apply to living organisms? Assuming that we know what these phrases mean, how could we tell that 'closure' on the perceptual level results in a minimum of configurative energy and not a maximum, or any degree in between? And since 'closure' is defined here without reference to the perceptual level, the definition must, if it applies to living organisms at all, apply to all behavioral levels. What can these esoteric phrases mean in behavior like thinking, speech, psychogalvanic reflex, and so on? Or in inanimate processes like the oxidation of iron molecules, spectroscopic dispersion, or planetary movement? Even if the definition could be shown to hold for physical processes, it does not necessarily have application to psychological processes. The 'definition' could undoubtedly benefit from a little definition itself.

Fuchs (**4**, p. 432) holds that completion (closure) can and does occur only if the seen part implies a whole of which it is a part—*i.e.*, whose law it already contains. This approaches a bit more closely an acceptable statement of what closure is, but it has some fatal weaknesses, namely, the words 'implies,'

'whole,' and 'law,' which remain entirely ambiguous or meaningless as used here. The word 'implies' seems to convey some notion of suggestion and conscious inference by the observer. Gestalttheorie, however, frowns upon the idea that consciousness is important in the perceptions with which they are concerned. If the word means that there are stresses or strains present in the part which inevitably lead to the subjective creation of the whole, then we land in the morass of undefined Gestalt terminology and remain ignorant of what stresses and strains can possibly mean in this context. The word 'whole' has, so far as I know, no delimited meaning, except in common parlance. According to Fuchs, the part 'implies' the 'whole,' and the 'whole' remains abandoned to our common sense comprehension, unless we can somehow fathom the meaning of the term 'law' in the phrase "the part implies the whole whose law it already contains." How can a part contain a law? What type of law can explain a perceptual whole? Is this law expressible? Is it mathematical? Physical? Electromagnetic? Vain questions, to which we can find no answers.

To Heidbreder who is outside the inner ring of Gestalt psychology, "'closure' is a special case of the 'law of pregnancy,' according to which experience, whether spatial or temporal, and whatever the sensory department, tends to assume the best possible form, so that forms tend to become more precise and more sharply defined—to become more completely, more typically, what they are" (**6**, p. 349). Intuitively, I would say that some of the implications of this statement are false so far as 'closure' goes; logically, I would say that it clarifies nothing.

In experimental work, the term 'closure' is usually used without definition or qualification of any sort, as if it were completely understood by, and acceptable to, all psychologists. Thus, Bobbitt (**1**) determines the threshold of 'closure' in simple geometric forms without bothering to tell us something about the phenomenon whose threshold he finds so interesting. Hertz (**8**) investigated the figural perception of the jay bird and found that the degree of 'closure' of the

stimuli was a significant factor affecting her results, but we look in vain for some enlightenment of the meaning of the term 'closure.'

It is useless to multiply references. The reader, if he so wishes, may scan the pro-Gestalt literature for a decent definition of the 'closure principle.' He will not find one.

It appears to me that we may draw a valid conclusion from the evidence adduced in this section. The phenomenon of 'closure' exists in perception. The term 'closure' merely designates the phenomenon as a valid problem for further investigation by psychology, but it does not explain it or render its conditions more comprehensible. 'Closure' has not yet been given adequate operational definition as a perceptual process, nor has it as yet been satisfactorily defined or identified on any other level. We cannot even be sure that it is correct to extend the term into any field other than perception, unless it be that of memory where there apparently is some, but far from conclusive, evidence for its existence (26, p. 80 ff.).

III

It will be the main purpose of this section to examine some of the invalid extensions or interpretations which Gestaltists have made of their as yet undefined 'closure principle.'

1. The attempt to explain 'closure' has lagged in the Gestalt literature. This probably arises from the attitude of Gestaltists that 'closure' is something given or inherent in the nature of things. Köhler's 'definition' indicates this attitude very nicely. Since 'closure' is held to be of this nature, we can understand the aplomb which Gestalttheorie manifests in employing the term without worrying too much about definitions. Gestalttheorie seems to feel that many of its terms are somehow inherent in natural processes, or inseparable from the functioning of an organism. Reading Köhler (15), one emerges with the frank admission that Köhler is interested in breaking down the wall between physics and psychology. The 'laws' he discovers are apparently believed to be of the same rigorous character as physical laws

like Boyle's Law, the inverse square law, or others. He is enamoured of the terminology of physics. It is this type of attitude which may be responsible for the fact that Gestaltists regard their terms as expressing phases of nature which are so basic as to be axiomatic, requiring no strict definition.

We do have, however, some instances where 'closure' has been approached from the physiological angle. The work of Fuchs (4, p. 347 ff.) apparently led him to the designation of 'closure' as a physiological process. Koffka has sanctioned this designation and says that "physiological processes take place both in the form of 'closed' and 'unclosed' responses" (9, p. 100). Moreover, from his work on hemianopic and hemiamblyopic patients, Fuchs claims that this physiological phenomenon is a central process, and not a peripheral one. Now, there can be no doubt that the perceptual phenomenon of 'closure' may be affected by various abnormalities of vision; and it may be argued that ultimate explanations of 'closure' should seek to be couched in physiological terms. But to take an as yet undefined phenomenon and to ascribe it to the central nervous system as a physiological process in itself, to take 'closure' and to explain it as a completion phenomenon in the central physiological processes—surely, this is conceptualization of the most precarious type, as Wyatt pointed out.

The perceptual phenomenon of 'closure' may have several explanations at the present stage of our knowledge. True, it is most likely that any or all of the possible explanations may involve central mechanisms. Yet, to allege that central processes are basic to a phenomenon, and to claim that the phenomenon is itself a completion process in the central nervous system, are two radically different things. It would, I imagine, follow from the theory of isomorphism that perceptual closure implies neural 'closure.' The principle of isomorphism itself lacks corroboration, and it may be that it is not a verifiable hypothesis in the first instance. However, we may say in the light of what we actually know of neural action, that it is almost impossible to conceive of a neural process as exhibiting 'closure.' A neural process may

come to an end, but 'closure' as found in perception is not merely arriving at an end.

2. Gestalt psychology has taken the term 'closure' and has extended it far beyond its original meaning as a certain type of perceptual phenomenon. We must not forget that 'closure' merely designates an as yet inadequately defined perceptual process. Keeping this in mind, it will be easy to see how farcical have been the extended uses to which Gestaltists have put the term. It has become a metaphor with which they toy, seeing its image and invoking its ghost in the most astounding regions. It recalls the ill-fated $S \rightarrow R$ terminology which came to the same ignoble end as a result of the incautious use of terms. The meanings of the words 'stimulus' and 'response' quickly burst beyond their operational limits. McDougall (**17**) pointed out the fallacy of applying 'stimulus' as a catch-all for anything which could evoke some behavior, and 'response' as the behavioral reactions to any of these so-called 'stimuli.' It is quite easy to show that an analogous disintegration of meaning has occurred with 'closure.' In the latter case, however, the effect is worse because we do not even have a valid definition of 'closure' whereas 'stimulus' did have an operational definition to start with. Evidence for this debasing of 'closure' into a metaphor is so common that we need only cite a few examples:

a. In memorization experiments, it is obvious that at the outset the subject does not have mastery of the material to be memorized. When he has passed through several stages of mastery and is finally able to recite the material without error, we say that the learning has reached a certain standard. However, to Wheeler (**25**, p. 293), the end toward which the memorizing of a series (of numbers) as a whole proceeds is 'closure.' He apparently identifies 'closure' with the mastery which the subject achieves.

b. Helson writes: "The concept of closure can be applied to thinking. Questions demanding thought arouse configurational processes which are incomplete and call for closure. The answer thus 'sticks' in the question. A good question

and a good answer are not matters of chance: they fit each other as the key and the lock. Once the form suggested by the question is apprehended, there is a sudden *Einschnappen;* the inner bond appears, baring the structure, and the configuration is completed. It is this sort of process which is responsible for the sudden drop in the curves of animal learning—a fact which trial and error, and frequency theories have never been able to explain" (7, p. 54). I count no less than twelve instances of undefined or ambiguous terminology, and muddled thinking, in this metaphorical obfuscation of the process of learning. This is not psychology, it is an invocation of the ghostly terms which Gestalttheorie has reified.

c. The 'closure' metaphor has been very ambitiously used by Koffka to explain instincts, behavior sequences manifesting purpose, etc. (9). Koffka states that instinctive activity becomes thus an objective mode of behavior analogous to such phenomena as rhythm, melody and figure. Köhler believes (14, p. 393) that it is just as intrinsic to visual structures that they should strive towards their own definite closure as it is for a human being that his behavior should proceed toward the intrinsically appropriate end of a behavior sequence. In the proclamation of 'closure' as the answer to psychovitalism, we may conclude that 'closure' is again used in its metaphorical sense: 'closed' = 'completed' = 'reached-the-goal' = 'instinct-motivated-behavior' = 'purposive behavior.'

d. 'Closure' is also the answer to the riddle of humor. The question of what constitutes a joke is a simple one to the Gestaltists. Harrower (5) performs an experiment based on the 'closure' principle, and Koffka (10, p. 616ff.) makes a critical use of her data in his discussion of thinking. Max Eastman (*e.g.*, 2, pp. 286–287), however, although he does not disapprove of Harrower's findings, fears that the Gestalt psychologists may over-emphasize the importance of pattern, and in the light of the previous discussion we find little difficulty in concurring with this opinion.

These examples will suffice to demonstrate how far afield we have been led. Let the reader try to recall at this point what the original meaning of 'closure' was, and he will see how removed these extensions are from the perceptual phenomenon which the term was meant to designate. Gestalttheorie faces an inevitable reaction if it permits this unrestrained use of its basic terms to continue.

IV

Summary: The term 'closure' was first used as the name for a phenomenon in perception. This phenomenon undoubtedly exists, and consequently the name 'closure' may validly be taken as denoting a problem which psychology must investigate. At present, 'closure' in perception cannot be, or at least has not yet been, adequately defined, but the attempt to define it formally as a perceptual phenomenon should logically precede the use of it as an explanatory concept on levels other than perceptual, if the latter use proves to be justifiable at all. Gestalt psychologists have been much too free in their use of the term, and have reduced it to the status of a metaphor which they loosely apply to a wide range of behavior. These extensions of a term which merely names an observed perceptual phenomenon are completely devoid of any real explanatory significance or logical validity.

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ON THE PROFESSIONAL TRAINING OF PSYCHOLOGISTS [1]

BY WALTER S. HUNTER

Brown University

The present national emergency is making extensive demands on psychologists for aid in a great variety of directions, demands that are revealing both the strength and the weakness of our personnel. There seems little doubt but that the number of psychologists directly engaged in governmental service will significantly exceed the number so involved in 1917–1918. The resulting applications of psychology, even though they may not be so widely publicized as were the Army Intelligence Tests of the last war, may reasonably be expected to stimulate future interest in the field and to have an effect on the coming academic programs of psychology and on its non-academic use. These aspects of the national emergency reinforce my belief that there is no more important problem confronting American psychologists as a group than that of the selection and training of their professional personnel. Perhaps also I have been sensitized to the problem through my long association with the National Research Council with its emphasis upon interdisciplinary coöperation and the opportunity which this offers for considering one's own science in relation to others.

It is therefore the broad and general problem of the selection and training of psychologists that I shall consider. We shall need to examine at least briefly very diverse aspects of the subject. We shall need to make clear what our conception of psychology is because this conception will very largely determine our procedures with reference to the specialized groups who must first master the fundamentals of psychology and then work in its academic or non-academic

[1] Presidential address of the Eastern Psychological Association, given at Brooklyn College, April 18, 1941.

practice. If in the course of the discussion psychologists find themselves in disagreement with the views that I express, I suggest that they regard this fact as an evidence of the importance of the questions that have been posed. There is no insistence on my part that the suggestions offered be accepted without debate any more than would be the case were we discussing the results of some experiment or the theoretical analysis of some problem. These reflections are offered to stimulate thought and action and not to close the discussion. Let us turn then to a consideration of the nature of psychology in so far as that may be necessary for the problem before us.

I assume that psychology is a science in the same sense that physics, chemistry, and biology are sciences. I further assume that the so-called social sciences do not in the same sense or to the same degree have this characteristic. I have no disposition to argue these points. I accept them as close approximations to present day realities without in any way implying that the natural sciences have nothing to contribute to the study of social issues or that they themselves are uninfluenced by such studies. It has been my privilege to serve with such National Research Council groups as the Committee on Work in Industry and the Committee on Problems of Neurotic Behavior as well as on various committees concerned with the current emergency. In these groups psychologists, personnel officers, medical men, physiologists, and engineers have been concerned with the help that the natural sciences can offer in the solution of significant social problems. It seems important to note that these men have not worked as social scientists; nor have they assumed that such fields as sociology and economics had nothing to contribute. They have worked as the natural scientists that they are, and any prestige that may attach to their labors derives from the thorough scientific training that they have had as well as from any insight that they may have shown. It is worth observing in the present connection, and in the present state of world affairs, that large numbers of people are well informed and reasonably competent to offer opinions

and interpretations with reference to many forms of social behavior and that the natural scientist and even the layman has much to contribute to the field, although the specialist in these areas may be the most competent single individual in certain respects. This situation is not new in kind although it may be so in degree. Intelligent men have always felt competent to discuss problems of morals, beauty, politics, philosophy, and religion, and now morale and propaganda, because they have had to deal with such problems and in so doing their opinions, often showing great insight, have been formed.

Psychology is a natural science whose findings, like those of its sister sciences, have important social consequences. Its subject-matter may be briefly designated as the behavior of the organism with particular emphasis upon sensory processes, general and special abilities, habit formation, verbal behavior, and personality. In studying such problems psychologists use experimental, statistical, clinical, and field observation methods which yield results that naturally differ in precision but that are vastly superior to anything derivable from uncontrolled observation. This fact-finding is of first importance, as it is in all science. It involves both the formulation of a significant problem and the utilization of reliable and valid methods for its solution. Laboratory experiments for the analysis of such specific functions of the organism as vision, hearing, and learning offer the best controlled instances of fact-finding, not only because the methods of securing the data are of considerable precision but because the concepts and hypotheses to be tested are also subject to clear and specific formulation. There are other fact-finding areas of psychology where the situation is much less satisfying because of the impossibility of securing the phenomena under laboratory conditions, the unclear nature of the concepts and hypotheses to be tested, and the difficulty of controlling the life-histories of the subjects. Difficulties of this nature beset work particularly in the fields of personality, motivation, child behavior, and social relationships and attitudes. The problems in these areas are often said to derive a particular

importance from the fact that they concern human behavior in the raw or as it occurs under natural conditions or from the fact that they involve the most significant aspects of behavior. However, if we were to accept such a point of view, we should go on and say that eugenics is the most important area of biology and psychiatry the most significant branch of medicine! This reasoning does not impress me as having great weight although I gladly concede the importance of the problems and areas to which reference is made. The factors which laboratory experimentation has shown to function in learning are also operative in behavior outside the laboratory; and it is at least debatable whether frustration is more important for the individual than vision or hearing. Two of the greatest areas in which psychological methods and results are involved lie in acoustical and illuminating engineering. Although few psychologists have infiltrated here, the reason is more the result of an academic orientation than of the alien character of the problems concerned. And we could repeat these statements for the fields of otology and opthalmology as well.

The suggestion has been made that laboratory fact-finding in psychology deals too largely with psycho-physiological questions and too little with vitally significant personal and inter-personal problems, that it is too much concerned with rats in mazes and too little with such topics as parent-child relationships. My only objection to such criticisms is the implication that certain problems are intrinsically more important than others. It is obvious that many areas in psychology have failed to receive the penetrating attention that should be given them. Scientifically, however, the importance of a problem should be determined by asking whether or not its solution will lead to the discovery of new factors or principles and whether the solution will affect a large or a small area of accepted facts and theories.

Laboratory fact-finding has been looked at askance by some psychologists on the grounds that it entails merely the addition of more minutæ and the picking up of more pins of knowledge. I think we must admit that in all science there

is a large amount of such work done under the hypothesis that a fact is a fact and should be recorded; but we should go further and maintain that such work typifies the universal trial and error procedure of prospecting for significant areas and problems as much as it illustrates the scientist's drive to be active on some even trivial problem. While the scientist works under the general hypothesis that ultimate social service as well as immediate intellectual satisfaction is the justification for his activities, it is asking too much to require him to keep this point definitely and continuously in mind. And it is a commonplace in the history of science that no one can indicate which facts will or will not be of great scientific and social importance in the future. What I have been saying should not be interpreted to mean that there is no fact-finding in such areas as personality, social attitudes, public opinion, and behavior disorders, although I think there is too little of it and too much interpretive writing. These problems represent relatively undeveloped and hence challenging fields for research, but they are no more important than the typical problems of experimental psychology. I am implying in addition that researches in these areas should follow a thorough mastery of experimental psychology rather than attempt to crowd such a mastery from the student's program.

The fact-finding or research function of the scientist is fundamental to everything else that he does since only such an activity can produce the data of his science. Few individuals, however, are in a position to devote themselves solely to research without rendering specific social service either in academic or non-academic fields. By scientific service I mean the utilization of a scientist's particular skill and knowledge for the solution of problems which do not directly involve the discovery of new facts or the formulation of new theories, problems which may be posed by such social groups as industry, government, clinics, and schools. Practice in such applied fields of science may involve the solution of genuine research problems, the discovery of new methods of work and of new limitations to laboratory findings; or it may involve an individual skill in the art of application. For example,

if a psychologist were given the problem of devising the best form of illumination for night driving which would at the same time favor the driver and handicap an airplane observer, the solution could only come from a careful study of the application of scientific knowledge to the particular conditions specified. If, on the other hand, the task were one of better adjusting a behavior-problem child or one of modifying the general level of group morale, the methods used would partake more of art and less of science; and wisdom and experience would count for as much as scientific knowledge.

The professional problem in which I am interested concerns the services rendered by psychologists to society and the training that is desirable for this work. The advancement of pure research is a service of the greatest importance, but it is one which needs no defense and we shall touch upon it only by implication while considering the selection and training of psychologists. We should gain little for our purpose by stressing the large number of areas in which the application of psychological methods and results are indicated; nor would it be advantageous to seek a classification in any terms of the problems met by the clinical, industrial, social, and child psychologists. On the other hand, we can make significant progress if we examine the character of the services which psychologists are rendering in undergraduate departments of instruction and if we then pass on to consider the nature of the graduate training which is available for those who will later work either in academic or non-academic fields. The psychologist like the chemist, the engineer or the physician is the product of his technical training, however much later experience may broaden and develop him.

Before turning specifically to the problem of undergraduate instruction in psychology, it will be interesting to compare the professional organizations of chemistry, physics, biology and psychology. Each of these sciences is concerned directly or indirectly with the training of college students, with the guidance of candidates for the master's and doctor's degrees, and with the development of men who will work professionally in applied fields; but only two of these sciences

have technical schools for the training of applied personnel. Applied physics is still largely in the care of schools of engineering, and applied (animal) biology is chiefly the function of the medical schools; whereas chemistry and psychology as a whole develop within the limits of college and graduate curricula. The trained psychologist or chemist working in applied fields is still known as a psychologist or a chemist. He gives his allegiance to the professional societies of these sciences, whereas the applied physicist and biologist tend to be absorbed into engineering and medicine with their different loyalties.

These facts have important consequences in the teaching of the several sciences. Schools of medicine and engineering not only maintain a rigorous course of study for their students but they reach over into the college departments of physics, chemistry and biology to specify a certain quantity and quality of pre-professional work that must be done. Thus the minimum pre-medical requirements are two to three years of chemistry, two years of biology, and one year of physics. While this seems a light requirement and while the medical schools from time to time emphasize the desirability of broad cultural training, the fact is that pre-medical students very largely major in biology or chemistry and enter medical school with a thorough foundation in science. Partly, I understand, as the result of professional unemployment during the depression, the American Chemical Society, through its Committee on the Professional Training of Chemists, now actually accredits departments as competent to offer bachelor's degrees which fulfill the minimum requirements adopted by the Society for the Professional Training of Chemists. It further accredits departments as competent to train for chemical engineering and for the graduate degrees.[2]

Of the sciences of biology, chemistry, physics, and psychology only the last has developed its academic program free from the influence of professional committees and professional schools. To be sure there is a much larger and

[2] American chemical society accrediting committee report. *Indust. and Engineer. Chem.*, 1939, 17, 592 ff.

more insistent demand for admission to careers in engineering, medicine, and chemistry then there is to a career in psychology. However, it is interesting to note that the number of doctor's degrees awarded annually in psychology in this country has increased from 91 in 1929 to 123 in 1939 and to 120 in 1940.[3] Furthermore forty universities in 1940 regarded themselves as qualified to offer doctoral work in psychology and granted from one to fourteen degrees each. The year 1940 also saw the award of the following numbers of doctor's degrees in other sciences: chemistry, 527; physics, 148; physiology, 70; and mathematics, 103. Psychology thus gave more doctor's degrees than did mathematics and physiology and gave almost as many as did physics, a relationship that has held for some ten years at least.

We all know that the number of psychologists represented in the membership of the American Psychological Association has grown by leaps and bounds. The 1928 Yearbook of the Association listed 534 members and 165 associates, a total of 699. Of this total Fernberger[4] calculated that 18 per cent were in clinical or industrial practice or research. Following 12 years of economic depression, the 1940 Yearbook lists 664 members and 2075 associates, a total of 2,739. A tabulation of the employment of these individuals in academic or non-academic positions indicates that 115 members (17 per cent) and 661 associates (31 per cent) are apparently employed full time in such non-academic fields as child clinics, mental hospitals, vocational guidance centers, and industry. The total group of 2075 associates is divided into 1275 holders of doctor's degrees, 25 per cent of whom are in full time non-academic work, and 800 associates holding lower degrees, of whom 44 per cent are similarly employed. These percentages would be very much higher if those individuals who give extensive part-time service to non-academic psychology were included. These figures should be thought of only as giving a general impression of the professional status of psychologists.

[3] D. B. Gilchrist (Ed.), *Doctoral dissertations accepted by American universities.* New York: H. W. Wilson Co. An annual publication.

[4] S. W. Fernberger. Statistical analyses of the members and associates of the American Psychological Association, Inc. in 1928. PSYCHOL. REV., 1928, 35, 447–465.

They should be replaced in the future with the results of a complete analysis of the records of psychologists included in the National Roster of Scientific and Specialized Personnel which has been prepared under the direction of Leonard Carmichael. Even the present statistics lend point to the question whether or not the time has come for psychologists, too, to take some steps toward the supervision of the training of professional psychologists. I shall offer no opinion on this question at the moment but shall turn instead to a consideration of the effects of such supervision in other fields.

Professional committees and professional schools influence undergraduate scientific programs in at least two significant ways. In the first place they increase the enrollment in certain designated courses by so sponsoring an approved curriculum that the support for such courses is not dependent upon free elections. It should be pointed out that relatively few students either major in biology, chemistry or physics or enroll in advanced courses in these subjects unless they are planning professional work in the field or unless such enrollments are a prerequisite to other professional study. This is very different from the situation in psychology where but few of the many undergraduates who major or enroll in the subject plan to continue professionally in the field. In the second place, an enrollment brought about by a professional agency whose scientific standards are high makes it possible to offer those thorough technical courses which are indispensable to the mastery of the subject but which are too difficult and technical to attract the average undergraduate. The absence of such outside pressure from professional psychology has made it possible for psychology departments to contribute more to the education of non-professional students than do the other sciences; but this contribution has been made at some sacrifice—in some cases at a very large sacrifice—of the standards of scientific instruction. If biology, chemistry and physics courses were unsupported by pre-medical, pre-engineer and pre-industrial chemistry students, their enrollments would be so small as to force either a contraction or a radical revision of their educational programs. Fortu-

nately, this is usually a theoretical problem for those sciences. For psychology the situation is a very practical one. In the absence of any well organized field like industrial chemistry at the B.S. in chemistry level or like medicine, psychology courses must either be made relatively non-technical and attractive enough to enlist a reasonable undergraduate support or, if the courses represent a progressive and thorough mastery of the subject, a definite curtailment of enrollment must be faced. The psychologist is thus between the devil and the deep blue sea! On the one hand he wishes to continue to make his very important contribution to the liberal education of the many students who seek contact with his field, and on the other hand his professional pride and standards urge him to teach his subject at the relatively high level found in the other sciences. The practical solution of course must be a compromise in which the highest quality of scientific instruction is given that is compatible with a reasonable enrollment when this latter is judged by enrollments in other science departments and not, for example, by enrollments in English and economics.

To attain such a goal there is no reason for psychologists to offer diluted and jazzed-up courses in literary science. My own experience has covered a variety of colleges, and I have no hesitancy in saying that, with a little pedagogical skill and enthusiasm on the part of the instructor, the most technical of elementary texts is not incompatible with a good enrollment of college freshmen and such a volume as Woodworth's 'Experimental Psychology' can be assimilated by sophomores and juniors in the experimental psychology course.

There is a large body of knowledge concerning the behavior of the human organism which is available for undergraduate instruction but which is largely passed over either through the fear of teaching something vaguely called non-psychological or through the fear that the general run of students will not take such courses in the absence of a vocational motivation. It is not generally accepted, but to me it is obvious, that if vision, hearing, emotion, or personality, are the topics for study, consideration must not be limited

merely to what are traditionally called the psychological aspects of the problem. One must utilize all relevant information whether it be from physics, engineering, medicine, physiology, or mathematics. The necessary thing is to understand vision or personality and not merely to present what the psychologists have done. This is so because no partial view of these interdisciplinary problems is adequate for their understanding. It naturally follows that at least an elementary knowledge of biology, chemistry, physics and mathematics is necessary if either the instructor or the student is to be able to read the literature of psychology understandingly. It is the failure to accept such a point of view that leads so many departments of psychology to neglect the broad scientific training of their graduate students. There are to be sure practical limitations to the establishment of prerequisites, limitations that particularly beset psychology whose undergraduate students are by and large uninterested in other sciences. Neither psychology nor biology can be expected to require extensive prerequisites for admission to their work at this level. Physiology, however, does better than psychology by rather generally prescribing a course in chemistry and by having in practice wide-spread support from the pre-medical students whose general program must include biology, chemistry and physics. Psychology can at least urge its better major students to take courses in the other sciences and in mathematics, and it can make such studies a prerequisite for any student contemplating later graduate work.[5]

I assume that no one will ask what difference it makes whether or not undergraduate psychology courses are held to a scientific level reasonably compatible with the available subject matter. It makes a great deal of difference in the intellectual training offered even to the general college student. It seriously affects the esteem in which psychology is held by the other science departments and so affects their willingness

[5] The omission of sociology, anthropology, and philosophy from the above list of subjects is deliberate, not on the grounds that these disciplines are unimportant, but on the grounds that when necessary the desired mastery of their subject matter can be secured, by reading outside of courses, to a degree quite impossible in the sciences.

to come to its aid in cases of academic crisis. It colors profoundly the conception of psychology which will be held by the future medical men, biologists, physicists, and engineers with whom professional psychologists must work and upon whose judgment they must depend for many professional opportunities. I dare say that much of the difference between the attitudes of these men toward psychology today and the attitudes prevalent twenty years ago is due to the improvement in the teaching of undergraduate psychology. And yet if one turns to the report (mimeographed) of the 1939 Round Table on the Elementary Course in Psychology, held at the annual meeting of the American Psychological Association, he will find this statement by one participant: "We tend to teach psychology from the point of view that 'the customer is always right.' We need to do this for 90 per cent of the students. Let them have what they want, but tell them it is not the real stuff." Fortunately this was not the prevailing view; and yet it is disturbing to hear the book-agents report on the large sale of texts which are written precisely from such a standpoint. We are inclined to laugh at the totalitarian countries that complain of over-population while urging their peoples to breed, and yet some psychology departments claim that their courses are too large for good scientific work while at the same time exerting themselves to secure more students!

Since psychology has developed its program of instruction without the guidance of professional committees or professional schools, it will be interesting to see what the undergraduate curriculum looks like and how much agreement there is between the different colleges. Table 1 is based on information published in 1938 by E. R. Henry.[6] Henry examined the undergraduate offerings beyond the elementary course in three groups of colleges: Group *A*, 48 colleges, 1500 or more students; Group *B*, 29 colleges, 1000–1499 students; and Group *C*, 80 colleges, 500–999 students. Table 1 lists only those courses which were offered by 40 per cent

[6] E. R. Henry. A survey of courses in psychology offered by undergraduate colleges of liberal arts. *Psychol. Bull.*, 1938, 35, 430–435.

or more of the departments in any one group. A blank entry does not mean that the course was omitted, but that less than 40 per cent offered it. Henry's original table has 25 entries, and he notes that in addition 34 other specialized and randomly offered courses were given!

An inspection of the table indicates that there is very little agreement among the colleges of Groups *B* and *C* where only 5 and 4 courses respectively are given by more than 40 per cent of the departments. In Group *A*, 9 courses satisfy

TABLE 1

UNDERGRADUATE COURSES IN PSYCHOLOGY

(Based on data gathered by E. R. Henry)

Subject Area	Per Cent of Departments Offering Course		
	Group *A*	Group *B*	Group *C*
Abnormal	81	75	61
Social	77	69	70
Comparative (Animal)	65	—	—
History-Theory	58	—	—
Physiological	54	—	—
Tests and Measurements	52	62	—
Applied	44	46	41
Child	44	—	—
Educational	—	41	50
Personality	41	—	—
(Statistics	39	24	9)
(Adv. Experimental	35	34	11)

this criterion, but no course is given in all departments. With all due allowance for the limitations of such a table, it is probable that not even in the colleges of Group *A* could the student be certain of securing either a good concentration in psychology or a fund of information comparable to that possessed by a student in another college of the group.

To compare with Henry's colleges, I have selected 14 Group *A* institutions all of whom have important graduate departments of psychology and have examined their undergraduate offerings, trying to eliminate from the calculation all courses offered primarily for graduate students. The results are found in Table 2. As should be expected, there is a higher agreement on more areas of work within these

professionally competent departments than is found in Henry's larger group. However, even these 14 departments vary greatly in the number of courses given in each area and in the number and variety of specialized courses which are difficult to classify. In some cases departmental offerings seem to multiply without much regard to the apparent general importance of the topic to be considered. This may be due to some insistent specialist in the department; but the assumption also seems to be present that, if the enrollment increases, new sub-topics should be taught rather than create

TABLE 2

UNDERGRADUATE COURSES IN PSYCHOLOGY
(Based on 14 colleges with important graduate departments)

Subject Area	Per Cent of Departments Offering Course
Experimental Psychology	100
Abnormal	90
Social	85
Tests and Measurements	71
History-Theory	71
Personality	64
Animal	57
Statistics	57
Child	57
Applied (Industrial)	57
Learning	50
Genetic (General)	42
Physiological	42
Esthetics (incl. art and music)	42

new sections in fundamental courses. In this connection it is significant that the professionally most distinguished departments of the 14 colleges in Table 2 offer the least variety of undergraduate courses.

Before turning to the problem of graduate training, I should like to suggest that the time seems ripe for psychologists to take some formal steps toward the improvement of undergraduate teaching. We are not ready to accredit departments as the chemists have done because there is no vocational pressure which can be brought to bear on our undergraduates. However, we could take steps to clarify the objectives and to indicate the minimum essential training involved in reaching them. Fortunately for the democratic process, no one

could compel adherence to such a report, but its guiding value should be considerable.

The problem of graduate work in psychology is at least three-fold: the goal to be attained; the curriculum and the general methods of training; and the selection and elimination of students. I am of course assuming that the staff is professionally adequate and research minded, and particularly that time and facilities are available for the development of a genuine graduate atmosphere.

Concerning the goal of graduate work there was formerly little or no disagreement in principle. The purpose was to develop scholars with some demonstrated research ability. Master's degrees were awarded but, at least in the institutions with which I was familiar, little professional importance was attached to them because of the absence of vocational opportunities in psychology for their holders. At the present time, the goal to be attained for the doctorate is the same as before; but the master's degree has assumed a large vocational importance in non-academic fields. This we noted earlier in taking account of the fact that 44 per cent of the 800 associates of the American Psychological Association who do not hold the doctor's degree are employed full time in such fields. So far as I can see from university catalogues, and so far as rumor goes, there is no specifically organized graduate training for non-academic psychological service either at the master's or at the doctor's level. The methods of attaining such goals can only be inferred. They cannot be read from the catalogues in terms of prerequisites and courses. I shall return to a discussion of this topic later (p. 521 ff.).

In considering graduate training, we must again confront the problem of the curriculum. Beginning graduate students are likely to have had both too much and too little psychology. They will in general have had too many courses; and the courses, which as we saw earlier will have been taught largely for the general college student, will have had too little in them, too little both in content and in intellectual training. As undergraduate majors, students will have been permitted and even encouraged to take more than the bare minimum of

psychology necessary to satisfy the concentration requirements; and they will, partly as a result of this, have an insufficient acquaintance with biology, chemistry, mathematics through calculus, and physics. This total situation can rarely be met solely by the careful selection of incoming students because the defects of undergraduate training for professional psychologists are present in some degree in all universities. Graduate departments should require that the deficiencies in the related sciences be made up without graduate credit and that some additional work in these fields be taken. They should offer a reasonable number of courses open only to graduate students where rigorous scholarly work can be demanded. No amount of course work and no known methods of selection and training can guarantee that the product will be an eminent psychologist. On the other hand I have yet to hear of one who was handicapped by thorough training in his own and related sciences.

In 1937 Douglas McGregor [7] published the results of an inquiry on academic prerequisites for graduate work in psychology. This report was based on replies from 82 psychologists who had received the doctorate prior to 1931, one half selected as successful and the other half selected at random from the Yearbook of the American Psychological Association. One question asked the group was to indicate the desirability of pre-graduate training in each of nine subjects by rating them as necessarily prerequisite, desirable but not absolutely prerequisite, or neither desirable nor necessary. The differences between the two groups of psychologists on this question were so small that the data were combined. Table 3 reproduces much of one of McGregor's tables. In the 'necessary' column, it will be seen that 54 per cent so rate college algebra, 77 per cent statistics, and 76 per cent biology while calculus, chemistry and physics rate only 15 per cent, 16 per cent and 33 per cent respectively. However, by combining the necessary and the desirable columns, over 70 per cent are shown to regard undergraduate

[7] D. McGregor. Should there be academic prerequisites for graduate work in psychology? *Psychol. Bull.*, 1937, 34, 501–509.

training in these nine subjects as desirable prerequisites for graduate work. This table indicates the status of opinion as of 1937, and it agrees in general with the 1934 findings of the American Psychological Association's Committee on Standards for the Ph.D. degree [8] to the effect that there is more lip-worship given to the desirability of scientific training in preparation for graduate work in psychology than there is insistence upon it. McGregor gives other tables comparing the opinions of experimental, educational, physiological and

TABLE 3

ATTITUDES OF 82 PSYCHOLOGISTS CONCERNING PREREQUISITES FOR GRADUATE STUDY IN PSYCHOLOGY

(Data from D. McGregor. Figures in per cents.)

Course	Necessary	Desirable	Total
College algebra	54	42	96
Analytical geometry	20	51	71
Calculus	15	64	79
Statistics	77	23	100
Physics	33	60	93
Chemistry *	16	57	73
Biology	76	24	100
Sociology	18	56	74
Anthropology	12	62	74

* The per cent for chemistry is interesting when it is recalled that the biological science most closely related to psychology is physiology and that chemistry is a usual prerequisite for that subject.

other psychologists and concludes as follows: "This study provides evidence that the diversity among the fields of psychology is no obstacle to the specification of reasonable and uniform prerequisites for graduate work. . . ." (p. 509).

In place of regarding the profession of psychologist as one calling for a broad and thorough background of scientific training on the basis of which one may specialize while still sharing a body of training with the whole professional group, a scientific background which the layman assumes that the psychologist as well as the physician possesses, we have been content to turn out psychologists with the minimum science that the traffic will bear. Such a situation would probably

[8] Standards for the Ph.D. degree in psychology. *Psychol. Bull.*, 1934, 31, 67–72.

not have arisen had due regard been paid to the interdisciplinary character of psychological problems. When some colleague reminds me that many psychologists make no use of physics or chemistry, I remind him that this is also true of many biologists and physicians. I call attention to the value of these related sciences in screening out men who cannot assimilate the scientific point of view. And I suggest to him that the psychiatrist is actively and usefully engaged with social problems and that I have heard of no tendency to exempt the budding psychiatrist from the rigors of medical training on the grounds that he will some day be working in a child guidance clinic. The resolution of the conflict depends upon one's conception of what psychology is. If the psychologist is a natural scientist who may wish to bring his skill and knowledge to bear on social problems, the answer is one thing; if he is a mere social surveyor or a minor technician in a clinic whose title should carry no implication that he is a scientist or well versed in the ways of the human organism, then the answer is something else.

And so we come to the problem of selecting graduate students. The problem of the elimination of the unfit, while important and sometimes bothersome, need not detain us because we can offer no suggestion other than the obvious one that graduate students should be judged as to their future professional value in terms of high scholarship, research competence, and good personality. I see nothing to be gained and much to be lost by permitting a student to continue his training when every aspect of his personality indicates that his only future will lie in some improbable research appointment where he could work free and alone. From the standpoint of professional success the lack of research drive and ability is much less serious than the lack of a reasonably well adjusted personality. The suggestion is not that scientific standards be relaxed but that there should be added some serious consideration of the personality of the candidates, some consideration of their ability to cooperate and to show good judgment and initiative. While from time to time some genius may appear who lacks these good traits and who should

never the less be encouraged, these cases will be so rare in anyone's experience as to be negligible from the standpoint of policy. It should be emphasized that admission to graduate study and continuance therein is a privilege extended to the student and not a right which he can claim.

At the present time the graduate school is dependent upon such evidence of a prospective student's ability as a transcript of course credits, letters of recommendation, and a record of academic honors received. This evidence has but uncertain value where knowledge of the institution from which the applicant comes is unavailable and where the authors of letters of recommendation are personally unknown to the graduate staff. Furthermore no transcript will reveal how much a student may have learned or retained from the courses taken, nor will it show how much he may have learned in a given field without definite course work. These shortcomings of the currently used records are in process of being met by a series of examinations sponsored by the Carnegie Foundation for the Advancement of Teaching. During the past few years, these examinations have been given to entering graduate students at Brown, Columbia, Harvard, Princeton and Yale and more recently at Iowa, Michigan, Minnesota, Wisconsin and perhaps elsewhere.

The examinations are of two types, a General Record Examination which covers in varying degrees all of the fields of a liberal arts education and an Advanced Subject Test in the student's field of specialization. Each examination is of the objective type and together they occupy two half-day periods, a total of some 6 hours. Figure 1 shows the average performance on the General Record Examination for entering graduate students (Columbia, Harvard, Princeton and Yale) in biology, psychology and sociology. The chart is so arranged that the average scores for all entering students, irrespective of their special fields, lie along the 500 line, and a distance of 100 points along the ordinate on either side of 500 represents one standard deviation. An inspection of Figure 1 indicates that the graduate psychology students in these four universities know, on the average, as much mathe-

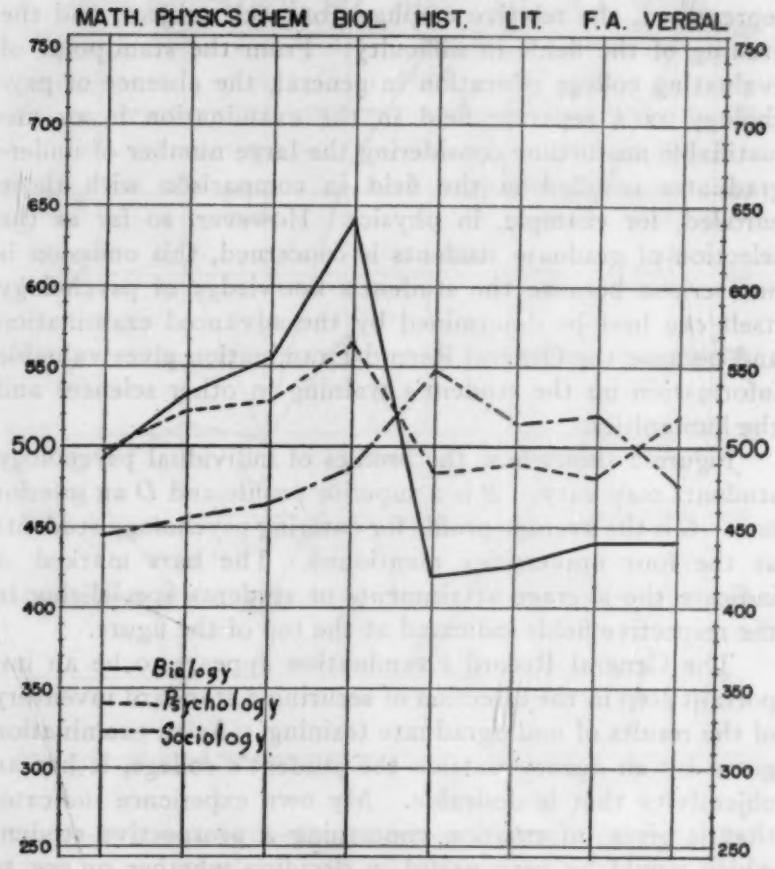

FIG. 1. Profiles on the Carnegie General Record Examination for entering graduate students at four universities in the fields of biology, psychology and sociology.

matics and physics and almost as much chemistry as do the biology students; and in addition they know more about the humanities. If the profiles for the other fields were entered on the graph it would be found that the physicists and chemists had a much less well balanced undergraduate education than either the psychologists or the mathematicians. And in addition it would be found that psychologists know more biology than any other group except the biologists themselves.

The General Record Examination is not in a finished state. It is in process of revision with respect to the subjects

represented, the relative emphasis on each subject, and the grading of the fields in difficulty. From the standpoint of evaluating college education in general, the absence of psychology as a separate field in the examination is an unjustifiable misfortune considering the large number of undergraduates enrolled in the field in comparison with those enrolled, for example, in physics. However, so far as the selection of graduate students is concerned, this omission is not serious because the student's knowledge of psychology itself can best be determined by the advanced examination and because the General Record Examination gives valuable information on the student's training in other sciences and the humanities.

Figure 2 shows how the profiles of individual psychology students may vary. *B* is a superior profile and *D* an inferior one. *C* is the average profile for entering psychology students at the four universities mentioned. The bars marked *A* indicate the average attainments of students specializing in the respective fields indicated at the top of the figure.

The General Record Examination appears to be an important step in the direction of securing a standard inventory of the results of undergraduate training. As an examination given by an agency outside the student's college, it has an objectivity that is desirable. My own experience indicates that it gives information concerning a prospective student which would be very useful in deciding whether or not to admit him to graduate work and whether or not to require him to take additional work in science. Should such an examination become established as a general guide in the admission of graduate students, each department would still need to decide what for it would be an acceptable profile. Obviously interviews or letters would still be needed as a basis for estimating the personal qualities and scholarly interests of the applicant.

The Advanced Subject Test, which requires 90 minutes, is designed to test the student's range of information in his own field. The validity of the examination is partly indicated by the fact that advanced graduate students score higher than

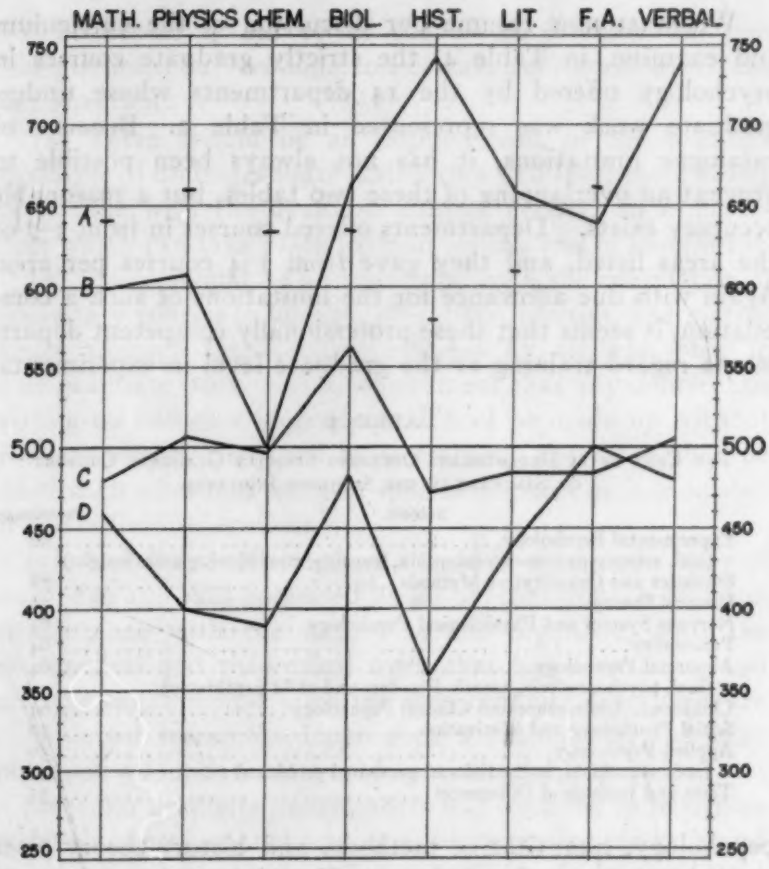

FIG. 2. Profiles on the Carnegie General Record Examination for entering graduate students. The cross-bars marked *A* indicate the average scores of students majoring as undergraduates in the fields designated at the top of the chart. Profile *B* is for a superior psychology student and profile *D* is for an inferior one. Profile *C* is the present norm for entering graduate students in psychology at four leading universities.

entering students and partly by the fact, at least in my own experience, that the score received agrees well with one's own estimate of the student after a period of personal acquaintance. (The scores on this examination are reported in the same numerical units as before.) Although my personal experience is limited, I am inclined to believe that there is no significant correlation between the profiles of the one examination and the scores on the advanced test.

We must now resume our discussion of the curriculum and examine, in Table 4, the strictly graduate courses in psychology offered by the 14 departments whose undergraduate work was represented in Table 2. Because of catalogue limitations, it has not always been possible to prevent an overlapping of these two tables, but a reasonable accuracy exists. Departments offered courses in from 3–7 of the areas listed, and they gave from 1–4 courses per area. Again with due allowance for the limitations of such a compilation, it seems that these professionally competent departments regard training at the graduate level in experimental

TABLE 4

PER CENT OF 14 DEPARTMENTS OFFERING STRICTLY GRADUATE COURSES OR SEMINARS IN THE SUBJECTS INDICATED

Subject	Percentage
Experimental Psychology (incl. sensory processes, perception, learning, conditioning and thought)	90
Statistics and Quantitative Methods	78
History-Theory	71
Nervous System and Physiological Psychology	64
Personality	64
Abnormal Psychology (incl. psychoanalysis, speech disorders and child psychiatry)	64
Childhood, Adolescence and Clinical Psychology	64
Social Psychology and Motivation	50
Applied Psychology (incl. vocational, industrial and personnel problems)	50
Tests and Individual Differences	35

psychology, quantitative methods, and history-theory as of great importance; but it is not clear that such work would be required of all students even in the departments offering the courses. This plus the fact that the percentages are below 100 indicates that no one fundamental body of knowledge will be canvassed at the graduate level in all 14 of these universities. The re-appearance in Table 4 of many of the same areas of study found in the undergraduate curriculum seems to indicate a general appreciation of the inadequacy of the undergraduate training upon which later work must be based. But the diversity of offerings, which caused us some concern in the case of undergraduate work, here becomes an indication of a legitimate diversity in specialization at the strictly professional level.

The preceding survey of the training which the psychologist must survive indicates certain deficiences which call for correction:

(1) There should be an improvement in the scientific standards of undergraduate instruction to a level more nearly compatible with the available subject matter, an improvement which can take place without serious loss in enrollment.

(2) There should be an insistence upon the completion of elementary courses in biology, chemistry, physics, and mathematics through introductory calculus by all who plan to enter graduate work, plus a requirement that any deficiencies existing on entrance to graduate school be made up without graduate credit. To such basic training, there can then be added such advanced work in specific sciences as is indicated for the individual student.

(3) And above all else there should be a common body of knowledge and skill possessed by all psychologists, a common acquaintance with the nature of the organism, with the experimental and theoretical work that has been done, and with the fundamental scientific methods and tools that are available for research. Upon such a foundation, and only upon such a foundation, should specialization proceed.

Since our graduate departments will continue to recognize two levels of professional attainment by granting master's and doctor's degrees, and since many individuals at each level will enter non-academic service, departments should clearly indicate the integrated professional curricula desirable and available for such ends. It would be helpful to have some general agreement on what constitutes the minimum acceptable preparation for professional work in the most important areas, for example, academic, clinical, and personnel psychology; but underlying such an agreement should be the assumption that any one worthy of bearing the title of psychologist should be first of all a scientist. My belief is that we cannot soon attain such ends by a completely *laissez-faire* policy. Only some central committee speaking with the prestige of organized American psychology or some collabora-

tion between the outstanding graduate departments concerned can hope to make the progress that is desirable.

We have much to learn from the field of medical education both with respect to the problems already discussed and with reference to post-doctoral training. As in medicine, so in psychology, the young professional student must be held to a mastery of the fundamentals and the interdisciplinary aspects of his field before he specializes. Psychiatry and psychoanalysis should not be invaded by the professional amateur devoid of basic training any more than social, clinical and industrial psychology should be, if the future interests of psychology are to be conserved. These fields are full of human interest and of the possibility of service, but they call more loudly for competence than for sheer enthusiasm.

Psychology might well consider the establishment of post-graduate interneships as a desirable or required stage in professional advancement. In academic circles we are already familiar with post-doctoral research fellowships and with the use of instructorships as apprentice stages in academic service. There is a widespread practice also of using graduate students as assistants and as occasional lecturers thereby giving them practical training as college teachers. More use could be made of half-time post-doctoral instructorships which would have the combined function of teacher training and research. Such appointments would be essentially interneships in academic psychology with a modest stipend of some $1200, and without promise of renewal. The customary full time instructorships fail to offer comparable advantages because of the burden of routine work usually imposed on the holders of these appointments.

In the non-academic fields of psychology much should be done to systematize and regularize interneship work. It is not enough that a graduate student spend a summer in some mental hospital, child clinic, or business organization, although these are not negligible opportunities for training. Graduate departments that train professional psychologists for service in such fields have a responsibility not only for the thorough

scientific preparation of their students but also for integrating this preparation with social service following the student's graduation. This responsibility is perhaps greatest to the numerous holders of master's degrees who are now associate members of the American Psychological Association and who are so largely in non-academic fields of work. Definite steps should be taken to encourage these individuals in their development as professional psychologists through the organization in a few universities of summer courses or institutes where the advances in non-academic psychological practice would be stressed and where distinguished non-academic psychologists would be members of the staff.

Even if psychology were a science confined strictly within academic walls, it would be necessary to raise the questions which we have discussed. It would be important that psychologists have enough training in the related sciences to read the literature of their own discipline intelligently, that they share a common body of knowledge and skill among themselves, and that they be able to stand on a basis of relative equality with their academic colleagues. But psychology is not so confined. Psychologists work outside the universities in association with physicists, engineers, psychiatrists and others who rightly expect the psychologist to have a scientific background and competence equal or superior to their own. To insure our continued and improved service in such fields, let us take some thought and thereby add a cubit to our stature.

PRACTICE AS CAUSE OF REMINISCENCE [1]

BY HORACE B. ENGLISH

Ohio State University

AND

ALLEN L. EDWARDS

University of Akron

Reminiscence as the term is used in memory experiments is defined as improvement in recall or recognition scores without intervening practice. But how can there be improvement without practice? Such an assumption is repugnant to commonsense—that is to say, it is repugnant to the theory of learning by associative contiguity which has become ingrained in our cultural pattern. James, who alluded to reminiscence when he spoke of learning to skate in summer, attempted to explain it as due to unconscious consolidation of neural traces—but although this speculation has been adopted by many others, it is clearly only giving name to our ignorance.

Other investigators have suggested that what appears as reminiscence may be, in part, due to the practice which is involved in the experimental set-up itself. Typically, this experimental set-up has called for two or more measurements of the amount learned, one relatively immediate, the other delayed; and the average improvement from the first test to the later one has been taken as the measure of reminiscence. Thus the first test is viewed as a sort of rehearsal or review and as such constitutes a practice period for all subsequent tests. Some of the improvement, when it is found, may therefore reasonably be ascribed to this practice.

In 1939 Edwards and English (1) published a brief experimental study in which they reported evidence that the

[1] Part of this paper was presented at the December, 1940, meeting of American Association for the Advancement of Science at Philadelphia.

immediate test does serve to increase certain types of scores (summary) on later tests, but that there is also recognition-reminiscence even when there is no immediate test. More recently Susan Gray (4) has attacked the same problem with recall. With respect to her findings it is worth noting, in the first place, that where the learning task required a greater degree of organization of the material, there was a greater degree of improvement from the first to second test. Though less striking, her results in this case are congruent with our own series of studies in substance learning.

Her chief conclusion, however, concerns the relation of practice to reminiscence. Finding, as we had also with recognition, that delayed recall preceded by immediate recall is better than recall not so preceded, she concludes that the additional practice involved in giving the immediate recall trial explains, or rather explains away, the improvement. It is this point which we wish to examine critically.

Gray did the perfectly typical thing. She took the median scores on immediate and delayed recalls and looked for gain or loss. Gain in the median score is called reminiscence. If it can be accounted for in terms of rehearsal or practice afforded by the immediate recall, then reminiscence is explained away. (Loss in the median from one test to the next is called forgetting and of course needs no explanation!) But suppose we ask *what it is* that is being practiced. Not a median surely. No, the subjects were practicing making certain responses. 'Certain responses,' you say? 'Which ones?' It is not until we have asked this question that we have really approached the psychology of the situation.

Now at the risk of stating the obvious, we must insist that one usually practices *only the responses which one makes.* If the immediate recall is to act as a practice-review, surely it benefits only—or at least primarily—those responses which get made. Any other interpretation is going to involve us in some modifications of the usual meaning of practice.

We shall turn, therefore, to the responses as actually made. Let C stand for a correct recall, I for an incorrect recall, O for a failure to recall, and let each symbol stand for

a *particular response* as operationally defined by the experimental procedure. If there are two tests, immediate and delayed, we get these possible permutations or classes of response-patterns.

Class	Immediate Response	Delayed Response	Interpretation[1]
1	C	C	Retention
2	C	O	Forgetting
3	C	I	Forgetting-Falsification
4	I	C	Reminiscence
5	I	O	?
6	I	I	Falsification
7	O	C	Reminiscence
8	O	O	No learning
9	O	I	No learning-Falsification

The immediate test provides practice of the response found in the column headed 'Immediate.' If, on the first test, the response is C, practice of this particular response should result in a greater proportion of cases falling in Class 1 (when responses on both tests are considered) at the expense of cases falling in Classes 2 or 3. In short, the immediate response being C may be expected to lessen the proportions of I and O responses on the second test. But this means only that one should not expect so much loss in retention from the first to second tests as might be found when the immediate test (and therefore practice of the C response) is omitted. It in no way accounts for *improvement* or *gain* from the first to second test—at least we find it difficult to see how it can. Improvement is found only in Classes 4 and 7. When an incorrect response is made at the time of immediate recall, the subject is practicing error without punishment or knowledge that it is error. Thorndike, Rock, Tuckman and others, in somewhat similar circumstances, have shown that when we unwittingly practice errors *we learn to make errors.* How, then, we ask, does practice of errors operate to bring about a correction of specific errors? And it is this which really constitutes the phenomenon of reminiscence.

[1] This interpretation is made according to the usual experimental procedure.

It is clear that we are here working toward a sharper definition of reminiscence. It needs it. Few phenomena, we submit, are well defined when defined in terms of averages of we know not what. We suggest that henceforth the term reminiscence be restricted to the correction of an erroneous *response* or the supplying of a previously missing response without objective re-presentation of the memory material or direct associative cues thereto. This definition is broad enough to include the literary use of the term but, more to the point, it seizes directly upon the actual characteristic phenomena of the experiment.[3] It is, if you will, merely a logical corollary of the definition that the practice effect of the immediate recall cannot at all affect reminiscence as it is evidenced at the time of delayed recall. But strictly logical corollaries are important—and the safest of all conclusions—provided they start from a sound base. And there *are* phenomena which correspond to this definition.[4]

It is interesting to note, moreover, that the most common alternative explanation of reminiscence is also contra-indicated by the same reasoning. We are often asked whether there is not some sort of unwitting rehearsal or incidental practice in the inter-test interval. Now it is next to impossible to prove that subjects do not ruminate about the memory material in the interval. Suppose that such rumination does take place. *One cannot, during the ruminations, practice the correct response without making it.* (Again not unless we shift ground as to what we mean by practice.) And if one does make the correct response following an incorrect response, the reminiscence phenomenon is here coincident with the beginning of practice, and therefore not its effect!

[3] Acute readers will not fail to note that we have linked together as reminiscence two discriminable types of phenomena: the supplying of a missing response and the correction of an erroneous response. Our excuse is that both certainly form part of what is now experimentally treated as reminiscence; but they may be really distinct. Perhaps only the former belongs in the study of memory and the latter belongs in that still largely unexplored area, the psychology (as contrasted with the statistics) of error.

[4] See particularly (2) and (3) where such phenomena are rescued from the usual blurring by averages and shown to occur with greater than chance frequency.

Well, it is always interesting when the logically certain fails to be the obvious. And we surely cannot be accused of insisting on the obvious when we deny a hypothesis which has gone unchallenged for over twenty years and which has been adopted by many a distinguished scholar and investigator.

We believe the point has been missed because most psychologists have been excessively addicted to an uncritical use of averages. Stated in abstract and general form, this sounds distressingly banal. And yet, is it not true that the first thing we do when we get a collection of psychological data is to compute an average? And we make these averages the basis of our manipulations and of our thinking. That, we submit, is wrong.

The basic datum for psychology must be recognized as being a response—a specific, singular, particular response. That datum must be observed and measured. And it must also be scrutinized, interpreted, understood, made to yield up its particular psychological meaning for the problem under consideration. And this must be done with each response before any other operation can legitimately be performed upon it. We cannot assemble data into groups, we cannot average or correlate or do any other such operations except in terms of significant identities for the purpose in hand. Such significant identity cannot be lightly assumed.

Moreover, when we have taken more pains to establish the psychological significance of the individual responses, there will be fewer occasions for averaging. As we have pointed out elsewhere (**3, 4**), psychology needs a wider use of the statistics of categories instead of central tendency. Specifically, cross-sectional analysis by means of averages, that is to say analysis by averages whenever change with time is involved seems to us as dangerous in experimental work as it has been shown to be in the study of growth and development. The unit datum—a response, or a person, seldom we think a function—needs to be followed through by itself in all its individual vicissitudes in time. Premature averaging distorts the picture.

Summary

1. Before we can ascertain the effect of practice upon reminiscence it is necessary to break down the data usually blurred by averaging.

2. Study of the data thus broken down reveals two facts characteristic of the reminiscence situation. This leads to a re-definition of reminiscence as 'the correction of an erroneous response or the supplying of a previously missing response without objective re-presentation of the memory material or direct associative cues thereto.'

3. Reminiscence as thus conceived cannot be explained in terms of practice.

4. Renewed attention is called to the necessity of critical examination of every datum—in psychology, of each individual or of every response—before it is subjected to statistical manipulation. Each datum must have its psychological status established *before*, not after, it is averaged.

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PHILOSOPHY AND PSYCHOLOGY[1]

BY ALBERT G. A. BALZ

University of Virginia

The Southern Society for Philosophy and Psychology is a symbol of many meanings. It reflects a long intellectual history—not of thirty-five years, but of twenty-five hundred. The Greek Fathers, who founded the Republic of Inquiry, perceived that inquiring men must inquire concerning man. This, they realized, must be done if the enterprise were to be comprehensive in scope. But equally they recognized that inquiry concerning man would condition as well as facilitate all inquiry. It is not less needed if science dare use the earth for an assault upon the heavens. The Stagirite wrote a *Metaphysica*. But he did not forget to write a *De Anima*. Both writings are difficult—but he who would study either would do well to study the other.

The Society, as symbol, represents a faith immanent within a tradition and we look with gratitude upon those who represented the faith by establishing this symbolic institution, and who have nurtured the conviction and cherished its vehicle. Did space permit, I would recite the names of those who founded the Society, of those whose membership came to an end with death, of those who gave many years of fostering association, and of those who held the responsibilities of office. To all of those we owe tribute. The Southern Society, however, is a living thing. It is then highly appropriate that we think with gratitude of those who remain even today the Nestors and Patriarchs of our order. They are the living embodiments of our symbol's meaning.

[1] Read by invitation at the celebration of the thirty-fifth anniversary of the Southern Society for Philosophy and Psychology, April 11–12, 1941, Washington, D. C.

I am grateful to Professor Frank A. Geldard for the benefit of many discussions concerning the scope of Psychology. Without holding him responsible in any way for the point of view implied in this paper, acknowledgment is due him because any merit it may possess is so largely the fruit of these discussions. I am also indebted to Mr. W. C. Trueheart for penetrating criticism.

This list of Patriarchs is not long—but whatever its length, the names of Herbert Sanborn and Knight Dunlap must be included. Over decades, rather than years, they have voiced the conviction for which the Society stands. Amid the disintegration of the ancient faith they have stood firm. To express the nature of the faith of Sanborn and Dunlap is at once a tribute to them, to all the Patriarchs of the Southern Society, and a fitting celebration of its anniversary.

History discloses, unhappily, that faiths are not easily precipitated into discourse. Faith, we are told by more than one defender, transcends the limitations of finite minds. We may apprehend, in a manner all too insufficient, the implications of a symbol. But symbols have a way of expropriating the results of the mind's labors, only to present to mind a new challenge. The defender of the faith re-creates the task in the accomplishing of it. Humility becomes him. If I venture to become a defender of the conviction which the Society represents, I do so with humility, and as subject to the reproof of the Patriarchs.

As with all faiths, a barren formula of conviction is easily worded. The Southern Society symbolizes the conviction that the task of Philosophy and the task of Psychology have between them very special and peculiar relations. As one Science among many, and merely as Science, whatever relations hold between Philosophy and Science will hold between Philosophy and Psychology. But this is not that which the Southern Society symbolizes. It represents relations that are unique. Between Philosophy and Psychology, so runs the conviction, are relations not found elsewhere. They are inquiries ordered to one another in peculiarly fecund ways. An explication of the faith, accordingly, must establish this special interrelation. Indeed, the defense of the faith against the infidel—and there are hosts of infidels in recent decades—depends upon the discovery and articulated statement of the interrelation. Now rational beings, especially philosophers and psychologists, do not lightly become infidels. There must be many difficulties of discovery and statement, otherwise there would not be so many unbelievers. Even

Saints and Patriarchs find it difficult to convert unbelievers—of mere spokesmen little can be expected.

In principle, the physicist, the biologist, the psychologist, the philosopher and their kindred, are pure minds confronting a subject matter. In fact, they are human animals, and so a part of the realm of things concerning which they seek to know. Thereby a large but special field of applied science is indicated. Medical science may point the way to health, and thus lead to more efficient activity of scientist and philosopher in their vocations. Physics itself may reveal something concerning the conditions under which even the physicist, since he is a thing amid things, must work in order to accumulate more physics. The sociologist might inform us concerning the total social conditions which most favor or most hinder the pursuit of inquiry. And above all, the psychologist may be of exceptional importance as an expert adviser concerning the use of sense and thought and hands as the instruments by means of which man, immersed in nature, seeks to know the sea of being in which he is immersed. Observation and thought must be controlled, it is said. Laying aside the function of logic as such, this field of control is a field of applied science. That it is applied science for the sake of more science, pure or applied, is inspiring but incidental. This field of application, it is true, is one in which psychology is of exceptional value. But it does not assign a unique position to psychology, and certainly does not define unique relations between philosophy and psychology.

On a previous occasion,[2] I discussed the problem of defining the subject-matter of psychology. It is quite obvious that the interpretation of our symbol, the Southern Society, cannot proceed without some definition of the function of psychology and of the subject-matter that corresponds to this function. This is all the more true because we seek to determine, not the relations between philosophy and any science, but rather the specific relations between psychology and philosophy. It might well be urged that definition of philosophy and its

[2] *Cf.* Concerning the subject-matter of psychology, PSYCHOL. REV., 1940, 47, no. 4.

subject-matter is equally necessary for the question. Cheerfully admitting the contention, I must equally admit that I do not know what to do about it. The definition of philosophy usually turns out to be a philosophy, and this in a sense that gives scientists occasion for mockery. To some degree, one conjectures, this occurs likewise in a science. Even physics, despite its high organization, is not understood in identical fashion by each physicist. And assuredly there is some truth in saying that there are as many psychologies as there are psychologists. In any case, it may be contended that reflection upon the nature and subject-matter of psychology, and upon the relation of this science to philosophy, is philosophical reflection, whether it be done by philosopher or by psychologist. Levels of inquiry are indicated by the facts. It is one thing for the physicist to inquire concerning atomic structure by means of a cyclotron, for the biologist to inquire concerning the problems of heredity; it is a different thing for the one or the other to inquire concerning the conditions and significance of their respective scientific pursuits. Whether or not they dislike the name, they are philosophers in function. Because of the very nature of inquiry, then, definition of philosophy is less pertinent to the present question than definition of psychology. Perhaps we might agree that philosophy is concerned with questions that, indeed, are asked, but which better-advised persons such as scientists do not deign to consider. Let us then assume that the scientists among us are in an imprudent mood, usually foreign to them.

We remain plagued, however, by the thought that philosophical thinking cannot be conducted in a vacuum. We propose to inquire concerning the peculiar relations of philosophy and psychology. To facilitate this, it is proposed to consider first the function and subject-matter of psychology. But can we even discuss the questions without assuming, implicitly or explicitly, what really amounts to a philosophical or metaphysical point of view? Candor compels me to acknowledge that some measure of metaphysical commitment is inescapable. This theoretical difficulty, however, can be

practically ameliorated. On the one hand, the special doctrines characteristic of types of philosophical positions may be neglected, just as we may neglect types of psychological interpretation. We cannot recapture the metaphysical and scientific innocence of Adam before he ate of the fruit of the tree of knowledge. It may be possible to approximate the comparative innocence of Aristotle. This worthy, posssibly to his advantage, knew nothing of psycho-physical parallelism, interactionism, sensationalistic subjectivism, panpsychism, the compounding of consciousness, behaviorism, Gestalt-psychology, and profound epistemological debates concerning whether red roses are really red. Aristotle, it seems, decided that the world of things is intelligible. Accordingly, he thought, broad statements characterizing the world could be made. These would be generally accepted and on that basis we could proceed to whatever adventures of mind might ensue. As far as possible, then, let us imitate the comparative disingenuousness of Aristotle.

The Stagirite would urge the advisability of establishing terminology—a terminology as philosophically and psychologically non-committal as possible. Let us follow his advice. In what follows, the term 'science' will be used to mean positive or descriptive science, in contra-distinction from what is sometimes called 'normative' science. Moreover, reference is made only to what is called the existential sciences. For the sciences of existence, it may be said, nature, the world, the whole of existence in time and in space, is so much matter-of-fact. All things and all happenings, past, present, and to come, all things and happenings that are existentially actual or possible, constitute the cosmic realm of matter-of-fact. The subject-matter for a particular science may be called the matters-of-fact for this science. Thus it may be said that there are matters-of-fact for physics and matters-of-fact for biology. There are many sciences, with differences, similarities, and relations too complicated for discussion. Some of these differences and relations are accidental—expressing historical and practical conditions. For the sake of a not unreasonable and Aristotelean simplicity, let us

assume that there are but four sciences of existential matters-of-fact: physics, understood as including astronomy, chemistry, and perhaps other divisions; biology and sociology, with appropriately similar inclusiveness; and finally, psychology. These four confront the total realm of matters-of-fact. Now there is a sense in which the matters-of-fact for one is distinguishable from that for every other science. There is a more important sense in which it can be said that each science owns, as its matters-of-fact, the entire domain of matters-of-fact. The first sense may be described as territorial, the second as cosmic.

The first or territorial sense implies some principle of selection by which jurisdiction is defined. I submit, without argument, that this selection is accomplished by the science itself. The history of a science represents—as I have elsewhere urged—a process that both accumulates, rejects, revises, and reorganizes. The net outcome, as of today or tomorrow, is a more or less coherently organized fund or system of ideas. The fact appears obvious. Such a fund is that which must be assimilated before one can be called, say, a physicist, and can pursue the inquiries proper to expertness in physics. This fund operates selectively upon the cosmic domain, and defines the territorial matters-of-fact for the given science. It is scarcely necessary to admit that the selective process goes on cumulatively, that expropriation of additional matters-of-fact leads to alteration of the selective fund, and that, finally, the altered fund determines new expropriations. Inadequately described, this is the territorial sense of a science's matters-of-fact.

The cosmic sense is less easily described. Indeed, it may not be entirely valid. There may be some matters-of-fact that simply cannot be expropriated by a given science. Descartes is the classic case of one who thought this. Psychical states, by virtue of what they are, must be excluded from matter-of-fact for physics. Descartes may have been right or wrong. Even if right, relations of correspondence and correlation may partially negate the exclusion. The possibility suggested, however, may be neglected in view of

our tentative standpoint of Aristotelean simplicity. How things and events are related to one another is something to be discovered. It would be presumptuous to lay down limits in advance of the movement of inquiry. There seems to obtain in each science an expansive thrust. In a sweeping degree, accordingly, the matters-of-fact for the major sciences tend somehow to coincide or at least to overlap. This fact is in one sense simple, even if analysis discloses unexpected complexities. What common sense and Aristotle would call a thing appears to be a nucleated system of elementary matters-of-fact. In consequence, one and the same 'thing' falls under the examination of several sciences—biology works away from living organisms to environing inanimate things and from the latter back to the living organisms; animate no less than inanimate things are seized by the tentacles of a science concerned with mass, motion, electricity, and chemical constitution. An obvious distinction must be drawn between the matter-of-fact for a science and the samples of such materials employed by those who pursue the inquiries appropriate to that science. The selection of samples is not merely a matter of theoretical import but also of practical convenience conditioned in many ways. In the light of these considerations, then, it is clear that the territorial areas can never be sharply defined. The boundaries, as they appear in a given stage of the history of inquiry, have a significance that is practical just as truly as it is theoretical. The truth of the situation seems to be this: In pursuit of what Aristotle would call the intelligibility of nature, each science widens as well as deepens its problems. Preliminary territorial claims as to matter-of-fact tend to expand and to approach the cosmic domain as a limit. Physics has not been content to remain the science of things sub-lunar. The intelligibility of nature implies its unity. Aristotle, I think, would regard this as a necessary assumption of inquiry. The scientist may be tempted to dismiss this as an irrelevant metaphysical proposition. In any case, however, what scientists actually do seems to imply a refusal to admit an ultimate irresolvable pluralism of nature until such time as the admission is forced

upon them. For present purposes this is sufficient. It is incredible that the several groups of scientists could ever be happy in the belief that the structures of ideas they rear should forever remain separated one from another. If this be true, it is a reflection of the historical facts. As the sciences develop, they seek differentiation from one another. But at the same time, they equally seek to inter-relate their several inquiries. The movement of the sciences indicates their tendency to expand in the direction of the cosmic whole.

On the grounds suggested, we may assume that there is a cosmos and that it consists of matters-of-fact. All distinctions between bodies of matters-of-fact, we further assume, fall within this cosmic assemblage. Many distinctions may be drawn—perhaps an infinite variety of them. Our present enterprise, however, demands distinctions relevant to the problem of the relation between psychology and philosophy, and yet these distinctions must be as philosophically and psychologically innocent as possible. What clue can we follow now? Common sense and Aristotle give an indication that psychologists and philosophers might accept in moments of naïveté. There are human beings. Human beings are things amid things. Things affect other things or are said to interact with them. Touchstone declares that it is the property of water to wet, of fire to burn, and that a great cause of the night is lack of the sun. With the innocence of Touchstone, let us then admit that things other than man affect men, and that men affect things other than men. Of course, it appears equally probable that things other than man affect one another, and that men affect one another. But for reasons to appear later, the latter probabilities may be neglected. Now, if things affect men, and men affect things, matters-of-fact come into being by virtue of this. If there were no human beings, or if human beings were purely intelligible substances like the angels, then certain matters-of-fact would not exist. Because men do exist, and because they are just exactly the things that they are, because they are things amid things which also are just what they are,

there is a class of matters-of-fact which result from these conditions. These are matters-of-fact whose existence depends crucially upon the presence and activity of men. However variable the other things in the interactive situation, the man-thing is invariably there. For the sake of a name, then, this class of facts may be called anthropocentric matters-of-fact.

Anthropocentric matters-of-fact naturally divide into several classes. It will forward our enterprise to distinguish them. There are such matters-of-fact as ploughs, domesticated cattle, mathematical formulæ on blackboards, the Panama canal, the songs of singers—the variety is well-nigh endless. Such matters-of-fact are due to human artifice—to man's powers of reaction, invention, making, doing, of incorporating something of himself in what he takes as materials for the incorporation. Such matters-of-fact would not be if men were not, and if their powers of action were not just what they are. To this class of anthropocentric facts the term, matters-of-artefact seems appropriate. It cannot be too greatly emphasized that all symbols, from language sounds to religious art, must be comprised within this set of artefacts. John Dewey [3] has recently taken the position that the use of symbols explains the admittedly extraordinary differences between man and the animals. Whether it does or does not, is not now the question. But Dewey's position indicates the overwhelming importance of this sub-class of matters-of-artefact. So far as one can see, anything and everything could be turned into a symbol. This could be done without material change in the matter-of-fact. When a scientist takes one matter-of-fact as a sign of another, there may well be a sense in which neither of these two matters-of-fact are altered. On the other hand, and in another sense, a profound alteration in the totality of matter-of-fact has occurred. The entire universe of matter-of-fact, potentially at least, can be taken over and converted into matter-of-artefact in the sense of symbols, and as we shall see, in another sense as well. It may be admitted—to

[3] *Logic, the theory of inquiry*, New York: Henry Holt, 1938, pp. 43–44.

assume a sophisticated position for the moment—that matters-of-artefact are veritably explosive with metaphysical and psychological problems. But however we may seek to avoid this sophisticated recognition, the incredible enrichment of cosmic matter-of-fact they bring must not be minimized.

Matters-of-artefact, including all actual and potential symbols, do not exhaust the facts that are due to man's compresence with the remainder of the universe. Objectively considered, a man amid things is just a thing amid things. Things interact, according to common sense and Aristotle. Things affect the human thing. In consequence, new matters-of-fact come into being. For common sense, it looks as if responsibility for each such coming-into-being must be shared by the human thing and the other thing. Suppose one had a fire; suppose further that one had a kettle of water. That is two matters-of-fact. If now the kettle be placed close to the fire, it may be that something happens. The water vaporizes. Here is a third matter-of-fact. Presumably, for common sense inspection, the water and the fire shared responsibility for the birth of the third matter-of-fact. In a corresponding way matter-of-fact comes into being when a human organism and some other thing are involved. Psychologists, one recalls, use such terms as stimulation, sensation, reaction, and many others when dealing with such matters-of-fact. But it is perilous for common sense and Aristotle to use these words. Let it suffice, accordingly, to say that the interaction of man as a thing with other things results in facts of the character suggested. The situation within which such matter-of-fact comes into being, and the matter-of-fact itself, are intricate. This is shown by epistemological controversy as well as by psychological inquiry. These matters-of-fact intervene, so to speak, between the scientist's inquiring activity—for he is an organism—and what we usually call the things outside and independent of the living body. The intervening matters-of-fact, seemingly dependent in their coming-into-being upon both organism and independent things, vary with changes in the former as well as in the latter. This set of intervening facts may be

called matters-of-factual-apparency, or, more briefly, matters-of-apparency. 'Apparency' is a word of dangerous associations. It arouses thoughts of epistemological and metaphysical contexts. Such associations should be neglected in order that the standpoint of relative innocence may be maintained. After all, it is a task of psychology to determine the meaning of 'apparency' in matters-of-apparency. Let it be assumed, then, that the anthropocentric situation, in which a man is a constant constitutive factor, while the other factors may vary, generates two vast realms of fact, the matters-of-artefact (including symbols and the use of materials as symbols), and the matters-of-apparency. A more far-reaching analysis would disclose the over-lappings of the two bodies of matter-of-fact, the need for subordinate divisions, as well as the intricate relations between all the divisions. A more extended analysis, however, is not required for present purposes.

The anthropocentric situation involves still another type of matter-of-fact that must be noted. The class is marked by the peculiar property of being two-dimensional. To indicate this fact, consider an illustration. It is the case that bigamy is matter-of-fact. It is equally the case that bigamy, in one society, is approved as an excellent state of matrimony, while in another society it is prohibited as a deplorable state of affairs. The approval and the disapproval are equally matters-of-fact. The fact is two-dimensional: Bigamy is a matter-of-fact, and the evaluations of it are equally a matter-of-fact. Such twice-born facts, coming into being in the anthropocentric situation, may be called matters-of-evaluated-fact. A sociological science, descriptively complete in aspiration, must describe all such two-dimensional materials. Now it must be noted that such description is not in itself normative, although the matters-of-evaluated fact described anthropocentrically come into being because of explicit or implicit normative activities on the part of human beings. The description as such does not attempt to evaluate, to determine the worth of, the inherent evaluations. It is one thing to say that in America bigamy is prohibited,

and another to say that it is rightly prohibited. It is one thing to declare that bigamy produces such-and-such consequences, and another thing to declare that the consequences are bad. It is one thing to determine what are the consequences of matters-of-evaluated-fact—which may be additional matters-of-evaluated-fact—and a quite different undertaking to evaluate the evaluations in terms of real or alleged principles of justice and the good. The discovery of such principles, and the comparison of actual evaluations with them, belong to some enterprise—ethics, perhaps—lying outside present discussion. Such an enterprise may be illusory, but the matters-of-evaluated-fact persist. The matters-of-evaluated-fact, accordingly, belong to, and perhaps constitute, the realm of sociological science. It is obvious, moreover, that they are comprised within the total perspective of biology as well. Man, at least up to the present, has survived. His evaluations, not less than his inventions, have something to do with his living and his surviving. It is conceivable that some day the biologists, gathered in solemn conclave, will advise us to live at peace in order that we may survive, to be highly virtuous in an honest society in order that we may not become extinct along with the dinosaurs, and to cherish our children in order that they may be effectively adjusted to the environment. When the biologists shall have given such counsel, we may recognize that they have expropriated the sociologists' matters-of-evaluated-fact, and have reckoned even with the admonitions of the ethicist. Perhaps this anticipated absorption of sociology into biology may come about. In the meanwhile, reflection must acknowledge the matters-of-evaluated-fact as among those arising in the anthropocentric situation.

It appears that the cosmos contains matters-of-artefact (including symbols), matters-of-evaluated-facts, facts-of-apparency, and everything else. 'Everything else' may be defined, negatively at least, as comprising all matters-of-fact that in no wise depend for their existence and specific characteristics upon the existence, the distinctive nature, and activities of man in the cosmic context of things. Indeed,

it might be asserted that cosmic matter-of-fact now falls into two portions, anthropocentric matter-of-fact and everything else. The very existence of this latter class might be questioned by assuming certain philosophical positions. Epistemological questions are prompted by the analysis. Psychological interpretations would develop the distinction into an intricate pattern. We are seeking, however, to define a point of departure. The effort to maintain a position of metaphysical and psychological innocence, however, yields these results as a common sense preliminary to philosophical and psychological reflection. To present this preliminary situation, with the least possible load of interpretation, is the whole aim of the analysis. If, indeed, the preceding discussion be regarded as an elaborate statement of a simple common sense point of view, it could be urged in defense that the point of view is a necessary beginning. In any event, two suggestions obviously emerge, and upon these depend the relations between psychology and philosophy.

In the first place, anthropocentrism, in the sense defined, is unavoidable. In every state of affairs in which man is a factor, anthropocentric matters-of-fact are implicated. Granting, with common sense, that cosmic matter-of-fact contains a vast realm of matter-of-fact whose existence is independent of the existence of men amid things, this realm is accessible only in and through anthropocentric matter-of-fact. The statement must be taken in a very simple fashion. Here, let us say, is a man pursuing physical inquiries. The inquiry, let us assume, terminates with a proposition such as this:—the sun contains sodium. Somewhere between the launching of the inquiry and its termination there occurred a spectral band of colors, with bright and dark lines. The inquiring man looked at and saw the spectral band. The culminating proposition purports to refer to matter-of-fact, *i.e.*, the sun's containing sodium, that is not anthropocentric. The common sense assumption, shared by physics, is that the sun, and its having sodium, are matters-of-fact the being of which is independent of the compresence of inquiring organism and other things. Metaphysical and epistemo-

logical reflection may *transform* the distinction between the anthropocentric and the non-anthropocentric matters-of-fact made by common sense. But the distinction, however altered, must be preserved in the transformation. In a similar fashion, psychological science may introduce distinctions between the stimulus and the immediate experience of the observing organism; it may disclose how extraordinarily intricate the situation really is; but the factual situation somehow remains, whatever the interpretations. In any case, and whatever may be the ultimate status of the cosmic matter-of-fact called 'everything else,' access to it is feasible only by way of matters-of-apparency. Indeed, the inquiry-situation, comprising the living man, his equipment of symbol and instrument, the sun-light and the sun, consists of a mystifying entanglement of matters-of-apparency, of symbol and artefact, and even of evaluated-fact. Pending metaphysical, epistemological and psychological analysis, the distinction may be accepted between anthropocentric matter-of-fact, with its indicated richness of character, and all other matter-of-fact which may be sweepingly characterized as non-anthropocentric.

From this interpretation a second suggestion develops. The distinction between psychology and other matter-of-fact science turns upon the distinction between anthropocentric and non-anthropocentric matters-of-fact. It is evident that the concern of psychology is primarily with the anthropocentric situation. But the interrelations between psychology and other major sciences on the one hand, and between psychology and philosophy on the other hand, are far more intricate than the distinction implies upon merely casual examination. It was urged above that, in a methodological sense, every inquiry is pursued *as if* the inquirer were pure mind, wholly disjoined from matter-of-fact. This obtains even with psychological inquiry. The method of doubt of Descartes was a device in part intended to explore this methodological character of inquiry. It makes no difference whether the inquiry be a scientific procedure or an ordinary exercise of intelligence, nor whether the inquirer be vocation-

ally a physicist, a biologist or a philosopher. The inquiry-situation, however, involves a human organism in all of its dimensions, whatever they may be. It is thus ineluctably anthropocentric in character. Psychology, however, cannot take as its matter-of-fact anythng less than all that man is and does, together with all that comes into being because of what he is and does. In this sense all scientific inquiry falls within the matter-of-fact for psychology. In a very important sense, the astronomer and his stars are not the concern of psychology. The nature of the stars is the astronomer's problem. The astronomer, in pursuit of his proper business, may seek the assistance of the psychologist in order, let us say, to avoid errors of observation. This application of psychology, however, is possible only because the astronomer and his stars, as anthropocentric fact, or as matter-of-fact related to the anthropocentric situation, are within the jurisdiction of psychology.

In revenge, however, the other major sciences include the psychologist and his matters-of-anthropocentric fact within their own jurisdictions. This is incidental to the point made previously, that the matter-of-fact for each science tends to approach cosmic matter-of-fact as a limit, and perhaps even attains the limit. The psychologist and his doings are within the jurisdiction of biology, should the biologist insist upon his rights. And more obviously, the psychologist and his activities are so much matter-of-fact for the physicist. Indeed, from this preliminary point of view, the entire anthropocentric situation with its intricate body of matter-of-fact can be claimed by the physicist. A plough is a configuration of materials for physics even if there would be no ploughs in the absence of human making. And man himself is not less than matter and motion whatever he may be in addition.

Two lines of consideration appear to intersect at this point. From the standpoint of our relatively innocent analysis, cosmic matter-of-fact falls into two portions, the anthropocentric and the non-anthropocentric. From the standpoint of scientific claims to subject-matter, alleged

jurisdictions overlap. Indeed, the claim of each tends to become all-comprehensive. From the one set of considerations, it would appear, anthropocentric matter-of-fact might be assigned to psychology, the remainder to the other sciences. We could even imagine these other sciences forming a single body of ideas, and so divide all matter-of-fact neatly between two grand sciences. But the second considerations seem to forbid such a simplification. The reconciliation of the opposition, it may be conjectured, would indicate in a preliminary fashion the exceptional relations obtaining between philosophy and psychology.

Matters-of-fact may be described as multi-dimensional. Each dimension is essential to the matter-of-fact in its concreteness of existence. In view of this fact, a dimension may be called constitutive. Descartes defined a constitutive attribute or essence of substance as something, in the absence of which, a thing could neither be nor be understood. Imitating and adapting this definition, a factor or dimension of matter-of-fact may be regarded as constitutive in that the matter-of-fact could not be what it is nor could be understood for what it is were the factor not present. Matters-of-fact, in terms of common sense, are similar and different in varying degrees. If all matters-of-fact without exception were alike in possessing one or more identical constitutive dimensions, then an inquiry devoted to these dimensions would indeed be all-inclusive. It would embrace the entire body of cosmic matters-of-fact.[4] This man, this animal, this stone, and this planet are each an intricately constituted matter-of-fact. An inquiry, based upon a single fund of ideas, can include all four with propriety if the inquiry be directed to those constitutive factors which are shared. Weight, temperature, space-time occupancy are common to them. Perhaps all matter-of-fact holds these factors in common. If this be the case, a 'science' whose existential reference is to these factors, would be truly cosmical in scope. It is to be especially noted

[4] It must be acknowledged that there are difficulties in the preceding statements. 'Similarity' and 'identity' of dimensions are expressions requiring further analysis, but this need cannot be met within the limits of this essay.

that all anthropocentric matter-of-fact would be included. In some such way as this a science such as physics may be defined as all-inclusive, and universal in this factual sense. This all-inclusiveness, however, is a consequence of abstraction. The question is not whether the man, the animal, the stone, and the planet belong to this or that science. The question is rather the sense in which they belong to a single science, to several sciences, or perhaps to every science. The inclusiveness due to abstraction, judged from a relatively unsophisticated standpoint, is accompanied by exclusion. The abstraction is accompanied by a systematic neglect of those constitutive dimensions not involved in the reference of the inquiry. A science such as physics appears to develop in the direction of a maximal abstraction and neglect, and towards a maximal inclusiveness.

It is necessary to observe an additional characteristic. The movement of inquiry towards maximal abstraction and inclusiveness engulfs anthropocentric-matter-of-fact. In so doing, however, it treats such matter-of-fact *as if* it were not anthropocentric. Now it appears not unlikely that anthropocentric matter-of-fact includes those cases in which matter-of-fact reaches its maximal richness of constitutive dimensions. Aristotle, one conjectures, would have said that a plant is a higher being than a stone, an animal than a plant, and a man than an animal. Translating this comparison into present terminology, man and the matter-of-fact with respect to which he is an essential component lies towards the limit of richness. If so, then man, when understood as matter-of-fact for physics, is the object of maximal abstraction in that science. Assuming with Aristotle that all things in nature are intelligible to some degree, there is suggested a science devoted to matter-of-fact of maximal constitutive richness. This, I submit, might suffice as a crude description of the task of psychology. In one way, because of the limitation of psychology to anthropocentric fact, its subject-matter is but a small portion of cosmic matter-of-fact. It lacks inclusiveness for two reasons. Its matter-of-fact is not merely limited to the anthropocentric

situation but is primarily limited to the ever-present factor, the human being, in the anthropocentric situation. Men are but a few drops in the sea of existence. At the same time the task of psychology is to understand matter-of-fact of perhaps maximal dimensionality. Its task is to understand them in their maximal dimensionality. As a consequence, no factor can be wholly neglected. It is not concerned, of its own right, with the fact that a man, a plant, and a star can be weighed. Just because it is engaged with multi-dimensional matter-of-fact in its integrated nature, and with the purpose of understanding it *as* integrated, psychology cannot sweep the whole sea of existence with its net. This is in no wise inconsistent with recognizing that there is a sense, as noted above, in which the astronomer and his stars lie within the matter-of-fact for psychology. In so far as non-anthropocentric matter-of-fact is accessible only through matter-of-apparency and of-artefact, in so far as the latter are dependent upon and related to the former, in that degree and in that sense even the subject-matter of psychology tends toward cosmic matter-of-fact as a limit. It seems improbable, for example, that psychology can accomplish its task without considering how the star affects the eye. It is, then, not at all surprising that psychology is so many-sided in its relations to and borrowings from other sciences. But psychology's movement towards the limit of cosmic matter-of-fact, like its borrowings, is merely expedient and auxiliary to its proper inquiry.

Granted that the preceding is acceptable, a further conclusion of extreme importance is suggested. This may be conveniently indicated by pointing to a possible fallacy. The limitations of psychology with reference to matter-of-fact are not necessarily an indication of limitation of significance. On the other hand, abstract inclusiveness in itself does not warrant the conclusion that inclusiveness supplies depth of insight. Assume, with Aristotle, the intelligibility of things. The degree of this intelligibility cannot well be determined beforehand. The scientists, it seems safe to assert, will admit that they have reached the limits of access to nature's

mysteries only if and when there appears to be nothing more that they can do. Now relative ease of access does not necessarily imply relative adequacy of apprehension of nature's intelligibility. Figuratively speaking, every science is a revelation of the nature of nature. It is not clear that the more widely shared factors of matter-of-fact are the most significant. Were it the case that every matter-of-fact without exception included weight, and were it the case that weight is understood, then indeed we should know something about everything. We should know something about the nature of nature. It by no means follows that in such knowledge we attained the very depths of the sea of existence. In a word, the revelation of physics may, in the end, turn out to be relatively superficial. Psychology, concerned with the most richly constituted matters-of-fact, may be in principle and potentially vastly more profound. It is possible, at least, that the very key to the Scriptures of nature may lie in the findings of psychology. The psychologists are often curiously humble folk. Their self-esteem has sometimes suffered from a superficial comparison with their brethren. There is something god-like about the physicist. With his cyclotron, he is like Jove, hurling thunderbolts, while the psychologist, somewhat like a stable-boy, tends his rats running a maze. Jove, however, is now but a tenth-rate god, and there may be more sound than substance in his revelations of the mysteries. In any case, nature is the context within which science proposes to account for the rat's behavior as well as for the thunderbolt. Who will deny the possibility that, in the long run, accounting for the former may provide the deeper insight, the more comprehensive knowledge of nature? Men are but drops in the sea of being. But there they are, in the sea of being, and by their unusual character they strikingly manifest the far reaches of possibility in the depths of that sea. If there are miracles, they are most exceptional matters-of-fact. If there are miracles, then piety is well-advised to regard them as profoundly revealing.

Psychology, then, must occupy a privileged position with respect to philosophical inquiry. Philosophy cannot neglect

the findings of any science. It must reckon with the results obtained when cosmic matter-of-fact is regarded all-inclusively and with maximal neglect of nature's incredible richness. But even more emphatically must philosophy consider nature in its maximal density of constitution, in its extraordinary and exceptional productivity. For this it must turn to the work of psychology. This science thus appears to be, for philosophical reflection, the most indispensable inquiry concerning existence. In so far as philosophy is concerned with existence, psychology beyond any other science reveals the resources of nature. If metaphysics be a legitimate enterprise; and if it attempt to understand what it means to be: then psychology, beyond the other sciences, indicates the depths of the sea of being that philosophical thought must sound.

It is sometimes stated, however, that psychology is the most backward of the major sciences. Even if this be true, preceding contentions are not affected. The psychologists may find comfort in the position of Aristotle. He pointed out that, with respect to inquiry concerning nature, we should start from the things which are more knowable and obvious to us and proceed toward those which are clearer and more knowable by nature.[5] We should distinguish between the intelligibility of things in themselves—in their own nature as such—and the degree of difficulty we experience in getting at this intelligibility. The intricate constitution of psychology's matter-of-fact may well imply that the pursuit of psychology is exceptionally difficult. But this does not warrant the conclusion that such matter-of-fact is relatively lacking in what Aristotle would call its intelligibility in itself. It may be in itself clearer and more knowable by nature, even if less accessible and less easily known by inquiring mind. The history of science may perhaps indicate that scientists, with practical good sense, began with that which is more accessible to us, with that which is more amenable to our ways of dealing with things. On another occasion Aristotle urged that the cause of a difficulty he was presenting might

[5] *Physics*, Bk. I, 184a. Oxford translation.

be, not in the facts, but in us. "For as the eyes of the bats are to the blaze of day, so is the reason in our soul to the things which are by nature most evident of all." [6] It is at least possible that existence is really most intelligible and revealing precisely in that matter-of-fact whose dimensional richness overwhelms the psychologist. Psychology may be backward, not because its materials have so little to communicate, but rather because they have so much to reveal to inquiring mind. All the more reason, then, why the philosopher should seek the company of the psychologist.

But why should the psychologist seek the company of the philosopher? This may be a more baffling question. The position of innocence, hitherto merely approximated and maintained with difficulty, is now jeopardized. So much depends upon what the philosopher thinks philosophy to be. It is prudent to answer the question conditionally. If it be granted that philosophy contributes to scientific activity by its criticisms and orderings of ideas, by the suggestion of unexplored possibilities; and if it be further granted that the undertaking of psychology has been acceptably indicated: then psychology, it seems reasonable to infer, should be in especial need of the services of philosophy. The richness of psychology's matter-of-fact, one conjectures, would require for exploration a corresponding richness of idea and hypothesis. If nature's intelligibility is maximal in such matter-of-fact, but if at the same time its accessibility to us is minimal, then an extraordinary array of tools and devices, intellectual and material, may be necessary in order to cope with the situation. In such conditions, philosophical criticism may well be exceptionally valuable. If the task of philosophical thought be that of attaining a global view of things, then all the more should such thought prove illuminating to psychology with its matter-of-fact at once so restricted, so internally rich, and so many-sided in its relationships. If we admit that every science rests upon a metaphysical foundation, whether acknowledge or unacknowledged, then we should expect psychology to be an especially tangled

[6] *Metaphysics*, Bk. Alpha, 993b. Oxford translation.

science. If metaphysics be an unusually stubborn effort to think clearly, as James said, psychologists could scarcely afford to neglect it. No one can question the fact that psychologists, like other scientists, feel compelled to return, after many and prolonged investigations of detail, to that accumulated fund of ideas which is pre-eminently his science. Reexamination, revision, and reorganization must ensue. Here the tasks of psychologists and philosophers might happily converge. The association of psychologist and philosopher may prove to be not unfruitful, for the one as for the other.

There are, one must admit, psychologists and other scientists who take the position that philosophical reflection has nothing whatever to contribute to the enterprise of science. There may be philosophers who are convinced that the *Metaphysica* can be constructed without the *De Anima*. But psychologists and philosophers of such convictions do not represent the tradition for which the Southern Society for Philosophy and Psychology serves as symbol and vehicle. That philosophers should seek the company of the psychologists, and psychologists of the philosophers, in the conviction that the association expresses something imposed by the very nature of things, this, I suggest, is the faith of the Patriarchs of the Society. It would be gratifying, indeed, were this *apologia* to receive their approval.

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THE PAST TEN YEARS OF PSYCHOLOGY IN THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY[1]

BY J. B. MINER

University of Kentucky

I. Introduction

When the Council of the Society asked me to follow up my paper at the 25th anniversary (38) by a survey of the last ten years, I wondered what possible use to science could be a survey of a particular geographical area of psychology. Then it occurred to me that enterprises receive their impetus from those who live in adjacent localities. Neighborliness has significance in instigating progress. It was not, therefore, a call to sectional prejudices and antagonisms. The uniqueness of this continued association of psychology and philosophy in one Society, this good neighbor policy, should generate some notions of real significance.

The total membership of the Society has increased from 138 to 274 during the decade, a sign of its vigor. We believe that in union there is not only strength, but breadth of thought. We think that this is a sign of southern hospitality, friendliness and perhaps conservatism. The American Psychological Association, by refusing to recognize us as an affiliated society, seems to regard it as perversity. Our answer is that conditions are sufficiently different in the South so that we no longer recognize membership in the American Psychological Association as automatically conferring membership in this Society. Last year, however, we stated that the Society "will continue to cooperate, as best it may, with every organization which intelligently aims to improve the status of scientific psychology and to interfere with none of them."

[1] Read by invitation at the celebration of the thirty-fifth anniversary of the Southern Society for Philosophy and Psychology, April 11–12, 1941.

Any historical treatment of psychology in the South should know of the recently reported study by Barkley (4) of the development of the subject at the University of North Carolina. In 1876, Psychology as a distinct course was taught there under its present title. This may have been the first time in the country in which this title appeared in a college curriculum.

The contributions from psychology to the programs of the ten years show that the University of Virginia has been the leading contributor, with 28 papers. Next in order follow: Johns Hopkins, with 20; University of Kentucky, 15; Vanderbilt and Peabody, with 11; Tulane and North Carolina University, with 10; Florida State College for Women, 8; Agnes Scott, American University, Duke, University of Georgia and University of Missouri, each with 6. Of the 402 contributions on the programs, 267 have been psychological.

Among the main supporting members to the programs have been Harry Johnson, with 10 contributions; Gilmer, 7; Dorcus, Dunlap, Lanier and Wallace, with 6 each; and Geldard with 5. During the last decade two southern psychologists, Peterson and Dashiell, have been presidents of the American Psychological Association, compared with two others, Dunlap and Watson, in the preceding 39 years. Another way of noting important southern psychologists is by the contributors to 'Psychology in Autobiography,' the 'Psychologies' of 1925 and 1930, the 'Handbook of Experimental Psychology,' and 'Readings in Experimental Psychology.' In addition to the presidents mentioned, we find major contributions by McDougall, Nafe, Stern, Harry Johnson, Dael and Helen Wolfle.

Some striking differences in relationships of southern psychologists to the national organizations are indicated by the fact that only 14 per cent of those in the Southern Society are fellows or associates in the American Association for Applied Psychology, while 75 per cent are members or associates in the American Psychological Association.

During the ten years the number of American Psychological Association psychologists with doctor's degrees who

are giving full time to teaching increased in the South from 66 to 242. The efforts of the Society's committee to improve the qualifications for teaching psychology in the South are getting results. Another committee has contributed, with the Midwestern Association, to studying the improvement of the first course in psychology. The number of bachelor's and doctor's degrees from southern institutions has tripled during the decade. This indicates an increased influence of southern training.[2]

II. Systems of Psychology

Let us go up in the air and take a plane view of the ground of psychology from a gas propelled machine. From the cross-breeding of psychology and philosophy we may expect some strange ideas which are characteristic of genius and eccentricity. From such prolonged group-thinking, we might even expect wisdom. When not only good fellows, but philosophers and psychologists, get together we find tolerance for

[2] Twelve mimeographed tables accompanied the paper. Copies may be ordered as Document 1598 from the American Documentation Institute, Offices of Science Service, 2101 Constitution Avenue, Washington, D. C., remitting 30 cents for microfilm form or 90 cents for photo-copies. For a limited time mimeographed copies will be supplied by the author. The tables showing the situation in the South bring up to date and supplement those published in the paper read by the author at the twenty-fifth anniversary of the Society (38). The contents of the new tables include: 1. Psychology budgets, staffs and students for one year including summer sessions; comparison of data from departments with salary budgets of $10,000 or more with those of less than that. 2. Increase in membership from the South in the American Philosophical Association and American Psychological Association. 3. Proportions of 208 psychologists in the Southern Society who are members of the American Psychological Association and the American Association for Applied Psychology. 4. Influence of the South on the training of American Psychological Association members. 5. Frequency of psychological and philosophical contributions to the Southern programs at each annual meeting from the 26th to the 35th, inclusive. 6. Comparison of papers from the Southern programs with the classified subjects in the Psychological Abstracts. 7. Methodological treatment in the psychological papers of the Southern Society. 8. Ph. D. members of the American Psychological Association giving full time to teaching at the beginning and end of the period. 9. Subjects of instruction of the American Psychological Association members in the South. 10. Changes in types of research problems of American Psychological Association members during the period. 11. Names of departments in which psychology is given, titles of teachers of psychology in the South; semester-credit-hours offered in different philosophical courses at the beginning and end of the period. 12. Distribution of courses in psychology in 1929 and 1939 in colleges, state teachers colleges and junior colleges in the South. Acknowledgment is hereby made of the assistance of Elizabeth Johnston and Paul W. Davis, graduate assistants, in the preparation of these tables.

any system of thought. So in southern psychology there have been radical behaviorists like Meyer (37) and Watson (51), structuralists like Nafe (39) and Geissler (16), mechanists like Peterson (42), responsivists like Dunlap (11), Gestaltists like Wheeler (56), topologists like J. F. Brown (5), purposivists like McDougall (30), personalists like Stern (47) and passivists like Dashiell (7), Geldard (20) and others.

At the beginning of this decade we were emerging from the *Battle of Behaviorism* (53). That wordy war has now passed into history, but it opened up new *Frontiers of Psychology* (35). Watson's departure from psychology (52) may have been one of the reasons why unadorned behaviorism has somewhat lost face, but the combat is still stimulating. Whatever one may think about hormic psychology (33), the personality of McDougall (32) endeared him to all. The beautiful memorial service in the Duke chapel left us with a prized memory of his influence. His autobiographical sadness at the slow reception of his telic position aroused our kindly feeling toward this man who is no longer alive to battle further.

Who can forget his picture of the violinist, which may represent the Waterloo of the strict objectivist. "I come into a hall," he wrote, "and see a man on [the] platform scraping the guts of a cat with hairs from the tail of a horse; and, sitting silently in attitudes of rapt attention, are a thousand persons. How explain the fact that the vibrations emitted by the cat-gut stimulate all the thousand into absolute silence and quiescence; and the further fact that the cessation of the stimulus seems to be a stimulus to the most frantic activity" (53)?

McDougall's systematic approach to understanding behavior was to take as a model for study the most complete, conscious, purposeful action. This was a challenge to psychology to study the directly observable upper level of conscious action. It might illuminate even the choice-point of rats. To those who are not blinded by pointer-reading operationalism, conscious goals are a pre-condition to important behavior.

McDougall also directed the attention of psychologists to their unusual opportunity to study biological emergents (31). If mutation is studied scientifically, it certainly manifests itself most frequently through original ideas. The conditions under which new ideas arise deserve more intensive study than has been given the problem. Another penetrating suggestion in McDougall's philosophical psychology was his attempt to formulate a new concept of 'directed energy' (34). That he left this still in mystical form provides an opportunity for the next generation.

McDougall's pupil, Westerhof, in his *Representative Psychologists* (55) has provided the most definite critical comparison of current systems of psychology which we have. Not only has he made a careful evaluation of purposive psychology, but his exposition of the Personalism of William Stern calls attention forcibly to a system to which the South, if anywhere in this country, may have a special claim. Stern's position (47) cannot be presented briefly, but the completeness and consistency of his personalism supplants other self-psychologies. His distinction between 'person' and 'thing' is almost classic. We can but regret that the fruitfulness of his system for investigation still needs testing by a larger group.

Taking a quite different systematic position as a structuralist, Nafe wrote at the beginning of this decade, "Purposive conceptions still find expression . . ., but their authors lose caste quickly among all psychologists . . ." (39, p. 128). To him it appears that there is "a tacit assumption by experimental psychologists as a group of a mechanistic hypothesis" (39, p. 128). Explanation for him means only "correlation with the facts of psychology and neurology" (39, p. 129). "With the shift of emphasis from systematic considerations to experimentally observed facts, the distinctions between schools of psychology have tended to disappear, . . ." (39, p. 129). Structural psychology is opposed to functional concepts. The Aristotelian problems of sensation, simultaneous and successive association he felt still embrace the field of psychology. Behaviorism and Gestaltism were side

issues. So far as they were experimental, they were still attacking the old problems in like ways, although under different names. "Polemics directed against systematic differences or imagined systematic differences of the experimentalists," he stated, "if not an excuse for not working, serve effectively to prevent our principle efforts from being directed to that end and comprise by far too large a part of our literature" (**39**, p. 139).

For Dunlap scientific psychology "steers clear of both mechanism and purposivism as these 'isms' are preached" (**11**, p. 311). Classifying his position as Response Psychology, he "discarded the old stimulus-response viewpoint" and recognized "integration as the cardinal process" (**11**, p. 312). The "scientific psychologist has no philosophical interpretations" but is interested in determining experimentally how the mechanism works and what part purposes play. He rejects the absolute determinism and the crude materialism of the mechanist as well as the supernatural element of a purposivist. He sees "no profit in assumptions which do not lead to experimental test" (**11**, p. 312).

Since Kansas is within our limits, we can claim at least one avowed Gestaltist. Wheeler's system (**56**) may not be regarded as orthodox by Wertheimer, Koffka, Köhler, *et al.* Nevertheless, near the beginning of the decade he attempted to adapt Gestalt concepts to a general text on psychology. At that time he said, "The science is being devitalized by an adherence to old problems and theories . . ." (**56**, p. xviii). His system is on the model of the magnetic field rather than that of the machine. If Gestaltists succeed in harmonizing Gestaltism, Purposivism, and Mechanism, we should be happy to recognize them as the great appeasers.

The latest systematic child born of the intercourse of psychology and mathematics is Topology. Here again the South has a neophyte in J. F. Brown (**5**). To the topologists one act is as good as a thousand for establishing an equation for vectorial action. They thus split science into two camps both of which, the topologists and the statisticians, are fighting for the cause of scientific truth. Each prefers its own

weapons. The protagonists may be fraternal but hardly identical twins. Perhaps pluralistic explanations in psychology may remain the ultimate solution as they are today in applied psychology.

Since no system has yet demonstrated its command of the field in any science, we may congratulate psychologists that they accept this fact more commonly than do any other scientists.

III. The Science of Science

The outstanding fact disclosed by the programs of the last ten years can hardly be said to be surprising in our cooperative society. In examining the proceedings, I have found that 60 papers out of 291 might easily be classified as dealing with the science of science, or, if you prefer, the philosophy of science. To cap this climax, the 1940 program was, I believe, the first in any psychological convention in which a symposium on the science of science has been scheduled. We may be proud of this when we notice that last year the national neurological and psychiatric associations devoted two full sessions to this topic.

If we follow Carnap (6), the science of science, in simple language, means the psychological origin of symbols, the logical definition and consistency of symbols, and the communicability of symbols. Six of the last ten presidents' addresses dealt with problems of this nature. Johnson (26) described 'Some Follies of Emancipated Psychology'; Winter (57) discussed 'The Postulates of Psychology'; Balz (2), 'The Metaphysical Infidelities of Modern Psychology'; Geldard (20), 'Explanatory Principles in Psychology,' and ten Hoor (49), 'The Philistines over Philosophy.' Although the target was the definitions and procedures of science, the arrows flew in so many directions that I shall not follow them in their flight.

In addition to these presidential addresses, we may note the titles of some characteristic papers: Dunlap (14), 'The Postulate of Common Content'; ten Hoor (48), 'Thought as Awareness and Thought as Behavior'; H. M. Johnson (25), 'The Logical Structure of Three Psychologies'; Sanborn (46), 'A Philosophy of Experiment'; D. M. Allan (1), 'Conscious

Functions and Cerebral Activity'; Weedon (**54**), 'Psychology as a Natural Science'; and Dwight (**15**), 'The Percipient in Perception.'

What is meant by psychology and by philosophy is of first concern to the Society. Two members have shown some concern about this problem. Geldard (**20**) is disturbed lest psychologists should not be allowed to seek psychological explanations in physiological, neurological, physical, chemical and pathological laboratories. Balz (**2**) is disturbed for fear that the psychologist may do so much visiting in surrounding elysian fields, that he will lose his way and cease to become any kind of scientist at all. Geldard believes that the house of psychology should be made from "any observation platform in which the planking has been cut from the same forest as that supplying psychology's footing. . . . Instinct, vector, response, purpose, redintegration, insight, regressive libido—all these terms and a thousand more, are names of 'explanatory principles' to the extent that they are genuinely descriptive of something" (**20**, p. 424). Moreover, he suggests that "perhaps as cogent a way as any of classifying and evaluating our contemporary problems is with respect to the known facts of neural structure and function" (**20**, p. 422). By this standard he finds sensory and learning problems on the psychological heights.

Balz, on the other hand, seeks for clues which may better fixate a professional field for psychology among the sciences. He has made his delimitation of psychology most clear in his paper today. "It would be baffling to learn," he has commented, "that Psychology does not exist and that there are no psychologists. It would be appalling to conclude that our psychological associates are perhaps just anatomists" (**3**, p. 337).

Causation is another fundamental concept treated in the programs. It has received an overhauling by H. M. Johnson (**27**), who is commendably enamored by symbolic logic. He demonstrated that 'sufficient implication' logically harmonizes 'mnemonic cause' (a succession observed in the past), the dynamic equation expressing a cross-section of the present, and purposive or 'telic' cause. By this magic wand, the

advocates of the old-fashioned cause and effect, hormic, Gestalt and topological systematists can walk arm in arm, each seeking out laws for its own causal concept, knowing that, so far as sufficient implication is concerned, they may only be saying the same thing in different language: "It does not happen that Q fails if P happens, and it does not happen that P happens if Q fails."

IV. Experimental Discoveries and Hypotheses

While the conceptual background of science occupies a place in every experiment, experimental discoveries are really the meat and sustenance of science. To cover the range of experiments which have occupied the largest part of the programs of ten years is impossible. I can only illustrate their importance by a few which touch wide interests.

Reports of experiments on learning have been numerous and varied. Among them the work of Peterson was outstanding. It culminated in his president's address (43) before the American Psychological Association just preceding his tragic death from pneumonia on the western trip immediately following. Peterson always showed ingenuity in devising methods for investigation. His mental maze and the transfer of stacked discs afforded new methods for studying the solution of more difficult problems. Nobody has understood better the statistical problems involved in learning experiments. Very characteristic of his thinking was his mathematical demonstration that the elimination of blind alleys from the goal backward was explained adequately as the expectation of a chance result, so long as the organism could not go back after it reached the goal. His demonstration that frequency and recency are not selective factors in eliminating blind alleys is recognized as conclusive (41). By his evidence, pleasure and displeasure, even pain, are not primary causal factors, or stimuli, for reaching maze-goals. The gradient hypothesis, likewise, has to meet the criticism from his analysis of experimental results.

Peterson's own fundamental hypothesis (43) to explain learning he called "completeness of response." Consumma-

tory responses are more complete, consistent and unitary than the conflicting impulses which are thus resolved. This may sound very much like the hypothesis of a dynamic field, but it is stated in terms of what is better known about the mechanism of nerve impulses, their reinforcement and inhibition.

The goal-gradient hypothesis must take account also of Robert's investigation of the swimming of rats in a circular pathway without cul-de-sacs (45). He demonstrated that the presence of progressive effects in gradients is a function of the arrangement of stimuli in the particular problem.

Dunlap (10), by evidence that undesirable habits are eliminated by practising them, brought motivation into the human learning problem more definitely than it had been before (13). He also introduced paralogs as a method for studying memory.

McGeoch, in the short time he was with us, opened up the problem of forgetting. His experimental results indicated that "perseveration must play a minor part, if it plays any part at all, in determination of the phenomenon of retroaction" (27, p. 55).

Dashiell has contended that 'set' as a fourth dimension in psychological research is "primarily . . . increase in readiness, and only by consequence is there any selecting or directing. The directing function of set is . . . neither universal nor primary" (8, p. 303).

With the coming of Nafe to Washington University, the South acquired the author to whom is commonly attributed the most sensational hypothesis in the field of sensation (40). Briefly stated, the different qualities of sensation have the origin of their differences explained, not in differences in sense organs, but in the volleys of temporal intensity and spatial patterns of discharge. While the theory was developed from the ingenuous investigation of feelings and cutaneous sensations, it has been generalized to cover the elimination of specific receptors from the explanations of all the sensory qualities thus far studied. The theory is a model of consistency.

Lanier's study (28) of the effects of denervation of his own cutaneous nerve cannot be overlooked in regard to any hypothesis about the skin senses. Although Head's theory

of protopathic and epicritic sensitivity was not confirmed, four separate afferent mechanisms were indicated immediately following the cutting of the nerve and during regeneration. These four afferent mechanisms fell into two groups: thermal and contact-pain.

Emanating at first from the interest at Virginia in sensory experiments there have been a series of papers on cutaneous and retinal responses by Geldard, Gilmer and Wallace. Each has appeared five or more times on the programs. Notable among these studies are those of the vibratory stimuli on the skin by Geldard (**21**) and by Gilmer (**22**); also on retinal contrast effects and binocular vision by Geldard (**19**) and Wallace (**50**). Geldard (**18**) also reported a very rare case of complete color blindness.

Dorcus in his presidential address on 'Research in Psychology in Colleges' (**9**), and Dunlap by his efforts toward a national institute of psychology (**12**), have directed attention to the need for better opportunities for research.

Interest has been revived in extra-sensory perception by the work of Rhine (**44**). For his followers, the stream of clear consciousness may dissolve into a mysterious canyon filled with mist which entices wayfarers into the Walhalla of clairvoyant prophecy in a timeless and spaceless universe. Whatever we may think of Rhine and his followers, he has certainly invited free criticism. Telepathy and clairvoyance remain a foundling on the doorstep of psychology.

It is not possible in this paper to include the many contributions in the fields of applied psychology, but mention must be made of the pioneering and outstanding work in child psychology of Buford Johnson (**23, 24**).

In conclusion may I express the hope that the close relationship which exists in the Southern Society may continue to make its unique contribution to both philosophy and psychology.

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